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Book Reviews

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Errata

Two pages were inadvertently omitted from the printed version of "Austrian Theorizing: Recalling the Foundations" by Walter Block that appeared in volume 2, no. 4 (Winter 1999). A corrected version of the article appears online at www.qjae.org/journals/qjae/pdf/qjae2_4_2.pdf. A downloadable errata can be found at www.qjae.org/journals/qjae/pdf/qjae2_4_9.pdf.

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DO ENTREPRENEURS MAKE PREDICTABLE MISTAKES? EVIDENCE FROM CORPORATE DIVESTITURES

PETER G. KLEIN AND SANDRA K. KLEIN

After a brief lull in the early 1990s, the market for corporate control became increasingly active toward the end of the decade. Both 1996 and 1997 set new records for the number of U.S. merger filings, and 1998, 1999, and 2000 brought high-profile "mega-mergers" in financial services, energy, telecommunications, pharmaceuticals, and automobiles. In banking alone, for example, a wave of mergers over the last decade has led to widespread industry restructuring and consolidation. While total industry activity continues to expand, the number of U.S. banks and banking organizations both fell by almost 30 percent between 1988 and 1997 (Berger, Demsetz, and Strahan 1999).

Like other business practices that do not conform to textbook models of competition, mergers, acquisitions, and financial restructurings have long been viewed with suspicion by some commentators and regulatory authorities. However, the academic literature clearly suggests that corporate restructurings do, on average, create value. Event studies of acquisitions consistently find positive average combined returns to acquirer and target shareholders. As summarized by Jensen (1991, p. 15), "the most careful academic research

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strongly suggests that takeovers–along with leveraged restructurings prompted by the threat of takeover–have generated large gains for shareholders and for the economy as a whole." These gains, historically about 8 percent of the combined value of the merging companies, "represent gains to economic efficiency, not redistribution between various parties" (Jensen 1988, p. 23).[1]

At the same time, however, several studies have found a sharp divergence between market participants' pre-merger expectations about the post-merger performance of merging firms and the firms' actual performance rates. Ravenscraft and Scherer's (1987) large-scale study of manufacturing firms, for example, found that while the share prices of merging firms did on average rise with the announcement of the proposed restructuring, post-merger profit rates were unimpressive. Indeed, they find that nearly one-third of all acquisitions during the 1960s and 1970s were eventually divested. Ravenscraft and Scherer conclude that acquisitions, particularly diversifying acquisitions, typically promote managerial "empire building" rather than efficiency. While acknowledging that product and capital markets eventually discipline poorly performing firms, forcing divestitures and other restructurings, Ravenscraft and Scherer (p. 217) argue for tighter government restrictions on mergers, particularly diversifying acquisitions and acquisitions financed by stock: "When the roads are strewn with wrecks, government officials cannot rest content because the tow trucks, ambulances, and hearses are doing a good job removing the remnants and clearing the right-of-way."[2]

Implicit in this criticism is the idea that divestitures of previously acquired assets expose past errors, and that these errors should have been *foreseen* (and perhaps prevented, if regulators had been sufficiently empowered). Certain types of acquisitions, it is claimed, are more likely to be later divested, so managers should generally avoid them. If such acquisitions occur, this is then cited as evidence for widespread agency problems. In this sense, the takeover wave of the 1980s is typically understood as an "undoing" of the earlier, conglomerate merger wave of the 1960s and early 1970s. According to conventional wisdom, the 1980s was a period of respecialization or "refocus," showing the failures of unrelated diversification. The three decades from 1960 to 1990 thus represent a "round trip of the American corporation" (Shleifer and Vishny 1991, p. 54).

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[1]On the gains from mergers, acquisitions, and other restructurings, see also the surveys by Jensen and Ruback (1983), Jarrell, Brickley, and Netter (1988), Roll (1988), Romano (1992), and Andrade, Mitchell, and Stafford (2001).

[2]Jensen (1986, 1993) argues similarly that diversifying acquisitions resulted from widespread agency problems in corporations, though he does not recommend any regulatory response: "The legal/political/regulatory system is far too blunt an instrument to handle the problems of wasteful managerial behavior effectively" (1993, p. 850). Instead, he advocates alternative forms of organization such as leveraged buyout associations and venture capital funds (see especially Jensen 1989).

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This view is based partly on evidence from studies of the conglomerate period by Rumelt (1974, 1982), Ravenscraft and Scherer (1987, 1991), Porter (1987), Kaplan and Weisbach (1992), and others who find no evidence that unrelated diversification brought long-term benefits to the firms that diversified.[3] Combined with evidence of negative stock-market returns to diversification during the 1980s (Bhagat, Shleifer, and Vishny 1990; Lang and Stulz 1994; Berger and Ofek 1995; Comment and Jarrell 1995), many observers conclude that unrelated diversification is per se inefficient, and that the conglomerate era is best understood as an agency phenomenon.

The conventional wisdom on conglomerate sell-offs can be challenged on at least four grounds. First, divestitures of previously acquired assets do not necessarily show that the original acquisitions were failures. Weston (1989) argues that divestitures occur for a variety of reasons, such as changes in corporate strategies and antitrust rules, and not necessarily poor performance. Kaplan and Weisbach (1992) studied 217 large acquisitions completed between 1971 and 1982 and found that while 43.9 percent had been divested by 1989, only about a third of those divestitures were responses to poor post-merger performance.[4] Thus the mere fact that many acquisitions are later divested does not prove widespread managerial misconduct.

Second, the market for corporate control is already highly regulated, and it is difficult to draw from current and recent experience strong conclusions about how unhampered capital markets would work. For example, Ravenscraft and Scherer (1987, 1991), Porter (1987), and other critics propose particular *sequences* of inefficient and efficient restructurings: diversifying, empire-building acquisitions in the 1960s and early 1970s, then efficient divestitures in the 1980s. But why did entrepreneurs make systematic mistakes during the conglomerate period, but not later? Can changes in the legal, political, and regulatory environments account for clusters of errors during specific periods?

Third, even if divestitures are seen as revealing prior mistakes, the failure of a particular acquisition does not necessarily indicate a failure of the underlying acquisition *strategy*. Certain kinds of acquisitions—for example, acquisitions of firms in knowledge-intensive, high-technology industries—may be

[3]Servaes (1996) also finds that conglomerate firms in the 1960s were valued at a discount relative to specialized firms. However, Matsusaka (1993) and Hubbard and Palia (1999) show that market participants rewarded conglomerate acquisitions during this period, and Klein (2001) offers valuation evidence consistent with the event-study results.

[4]Other empirical studies of asset sales and restructurings include Hite, Owens, and Rogers (1987); Lang, Poulsen, and Stulz (1994); John and Ofek (1995); and Schlingemann, Stulz, and Walkling (2000). These papers look at divestitures more generally, and not only at divestitures of previously acquired assets.

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inherently riskier than others. If the returns from a successful integration of the target's activities with the firm's existing activities are sufficiently high, then the acquisition has positive expected value, even if it is more likely to fail than a safer acquisition. Matsusaka (2001) offers this kind of interpretation of corporate diversification. Diversifying acquisitions represent experiments, as firms try various combinations of businesses, seeking those that match their capabilities (in the sense of Penrose 1959; Nelson and Winter 1982; and Wernerfelt 1984). After learning their capabilities, firms divest acquisitions that turn out to be poor matches. In this sense, divestitures reflect successful experiments–the acquirer has learned that the target's industry is not a good match for its capabilities. Such "match-seeking" firms will actively acquire and divest over time.[5]

This article elaborates a fourth, "Austrian" interpretation of corporate divestitures, one that builds on the three just mentioned. Austrian writers view market competition as a dynamic, rivalrous process that unfolds gradually through time–a "discovery procedure," in Hayek's (1978) famous phrase. The future holds genuine surprises, not merely a closed set of events whose probabilities are unknown. From this perspective, the long-term success of an acquisition, like any entrepreneurial action, cannot be "predicted." Entrepreneurs rely on judgment, or what Mises (1966) calls *understanding*. Understanding is intuitive, subjective, and qualitative, and thus inherently imperfect. For this reason, divestitures of underperforming subunits may be seen as efficient responses to unforeseen changes in industry and regulatory conditions, or more generally, to poor judgments by profit-seeking entrepreneurs. Ex post viability is not a good indicator of ex ante efficiency.

We begin with the theory of entrepreneurship proposed by Mises (1966), posing it as a challenge to the agency view of divestitures. We then present empirical evidence that the long-term performance of corporate acquisitions cannot, in general, be predicted by measures of agency conflicts. Instead, divestitures of previously acquired assets are more likely when firms are experimenting, learning, and otherwise trying to deal with uncertainty about

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[5]Sanchez, Heene, and Thomas suggest that the same is true for networks and alliances.

> In a dynamic market context, longevity of interfirm alliances is not necessarily an indicator of successful collaboration. A succession of short-term alliances by a firm, for example, may suggest that the firm has a superior ability to learn from its partners, or that it may have superior ability to quickly reconfigure its chain of firm-addressable resources in response to changing competitive and market conditions. (1966, p. 28)

Mosakowski (1997) also offers an experimentation theory of diversification (without looking at subsequent divestitures).

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future conditions. We also show that mistaken acquisitions are more likely under certain circumstances, namely during periods of intense, industry-specific regulatory activity. Our own research on restructuring (Klein and Klein 2001) shows that significantly higher rates of divestiture follow mergers that occur in a cluster of mergers in the same industry. As argued by Mitchell and Mulherin (1996), Andrade and Stafford (1999), and Andrade, Mitchell, and Stafford (2001), mergers frequently occur in industry clusters, suggesting that mergers are driven in part by industry-specific factors, such as regulatory shocks. When an industry is regulated, deregulated, or re-regulated, economic calculation becomes more difficult, and entrepreneurial activity is hampered. It should not be surprising that poor long-term performance is more likely under those conditions.

This last result is consistent with the view, expressed repeatedly in the Austrian literature, that entrepreneurial error is associated with government intervention in the marketplace—in particular, with government ownership of property and interference with the price system. Mises (1990) famously showed that economic calculation is not possible without private property in all markets, especially markets for factors of production. The Austrian business-cycle literature (Mises 1934; Hayek 1935; Garrison 2001) suggests that entrepreneurial errors are more likely under government-sponsored credit expansion. This article makes a related argument: Entrepreneurial decisions to make acquisitions that will later be regretted, and divested, are more likely in the wake of government intervention in particular industries.

The remainder of the article is organized as follows. The first section reviews the Austrian literature on entrepreneurship, uncertainty, and economic calculation, suggesting that the ex post success of entrepreneurial actions cannot be forecasted based on generally available information. The second section introduces recent theory and evidence on the reasons for mergers and divestitures, contrasting two opposing views of sell-offs: empire-building and experimentation. The third section reviews some empirical evidence on the pre-merger causes of divestiture, challenging the generally accepted, empire-building explanation. The final section concludes.

Entrepreneurship, Profit, and Loss

Entrepreneurship, as Mises (1966) conceives it, is the act of bearing uncertainty. Production takes time, so entrepreneurs must purchase factors of production in the present (paying today's prices, which are known), in anticipation of revenues from the future sale of the product (at tomorrow's prices, which are uncertain). Entrepreneurial profit or loss is the difference between these revenues and the initial outlays, less the general rate of interest. As such,

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profit is the reward for successfully bearing uncertainty. Successful entrepreneurs, or "promoters" in Mises's terminology, are those who make accurate forecasts of future prices and receive returns greater than their outlays. Those whose forecasts are less accurate earn losses. Promoters who consistently make poor forecasts quickly find themselves unable to secure any further resources for investment and eventually exit the market.

This entrepreneur-promoter is absent from textbook models of competitive general equilibrium in which uncertainty is defined away, replaced by probabilistic risk. In these models it is possible to anticipate which actions, on average, would be profitable. In a world of "true" (structural, rather than parametric) uncertainty, however, profit opportunities do not exist "out there," waiting to be realized by anyone willing to take a specified action. Instead, profit opportunities are *created* through entrepreneurial action. As emphasized in one strand of modern Austrian literature, entrepreneurial skill is not simply luck or "alertness," the ability to recognize profit opportunities that appear, *ex nihilo*, to the discoverer. Rather, entrepreneurship is *judgment*: "Alertness is the mental quality of being on the lookout for something new; judgment is the mental process of assigning relevance to those things we already know" (High 1982, p. 167). In this context,

> promoter-entrepreneurs are those who seek to profit by actively promoting adjustment to change. They are not content to passively adjust their . . . activities to readily foreseeable changes or changes that have already occurred in their circumstances; rather, they regard change itself as an opportunity to meliorate their own conditions and aggressively attempt to anticipate and exploit it. (Salerno 1993, p. 123; see also Hülsmann 1997)

All entrepreneurs, particularly those who operate in financial markets, use economic calculation as their primary decision-making tool.[6] By economic calculation, Mises means simply the use of present prices and anticipated future prices to compare present costs with expected future benefits. In this way the entrepreneur decides what goods and services should be produced, and what methods of production should be used to produce them.

> The business of the entrepreneur is not merely to experiment with new technological methods, but to select from the multitude of technologically feasible methods those which are best fit to supply the public in the cheapest way with the things they are asking for most urgently. (Mises 1980, p. 110)

To make this selection, the entrepreneur must weigh the costs and expected benefits of various courses of action, and for this he needs the cardinal numbers

[6]Klein (1999) argues that financial-market entrepreneurship is a particularly important form of entrepreneurial activity, though it has received little attention in the Austrian literature.

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provided by money prices. Monetary calculation, then, requires private property and market prices (Mises 1966, p. 229).

Mises's famous 1920 essay on economic calculation under socialism is not so much about socialism per se; it is an argument about the role of prices for capital goods (Rothbard 1993, pp. 547-78; Klein 1996). Entrepreneurs make decisions about resource allocation based on their expectations about future prices and the information contained in present prices. To make profits, they need information about all prices, not only the prices of consumer goods but the prices of factors of production. Without markets for capital goods, these goods can have no prices, and therefore entrepreneurs cannot make judgments about the relative scarcities of these factors. In short, resources cannot be allocated efficiently. In any environment, then—socialist or not—where a factor of production has no market price, a potential user of that factor will be unable to make rational decisions about its use. Stated this way, Mises's claim is simply that efficient resource allocation in a market economy requires well-functioning asset markets.

Despite Mises's explicit focus on entrepreneurship, much of modern production theory—indeed, the entire neoclassical theory of the firm—focuses not on entrepreneurs, but managers. The traditional theory of profit maximization is nearly always told from the perspective of the manager, the agent who operates the plant, not that of the owner, who supplies the capital to fund the plant. Yet owners control how much authority to delegate to operational managers, so capitalists are, in an important sense, the ultimate decision makers. To understand the firm, then, we must focus on the actions and plans of the suppliers of financial capital, the capitalist-entrepreneurs.

It is true, of course, that when capitalist-entrepreneurs supply resources to firms, they usually delegate to managers the day-to-day responsibility for use of those resources. The resulting possibility for managerial discretion is of course the focal problem of the modern literature on corporate finance and the theory of the firm. The literature on corporate governance identifies a variety of mechanisms by which shareholders can limit this discretion. Internally, owners may establish a board of directors to oversee the actions of managers. They can use performance-based compensation to motivate managers to act in the owners' interests. They can adopt a particular organizational form, such as the "M-form" structure, in which managerial discretion is more easily kept in check (Williamson 1975). Finally, they can rely on competition within the market for managers to limit the discretionary behavior of top-level management (Fama 1980).

Even more important are external forces that help align managers' interests with those of shareholders.[7] Competition in the product market, for

[7]Jensen (1993) argues that internal control mechanisms are generally weak and ineffective, while external control mechanisms—where allowed to function—are typically superior.

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example, assures that firms whose managers engage in too much discretionary behavior will fail, costing the managers their jobs. In countries where universal banking is permitted, large equity holders such as banks can exercise considerable influence over managerial behavior. The external governance mechanism that has received the most attention, of course, is the market for ownership itself, the market for corporate control. Anticipating Manne (1965), Mises (1966) makes the general point that financial markets play a vital disciplinary role:

> [T]he changes in the prices of common and preferred stock and of corporate bonds are the means applied by the capitalists for the supreme control of the flow of capital. The price structure as determined by the speculations on the capital and money markets and on the big commodity exchanges not only decides how much capital is available for the conduct of each corporation's business; it creates a state of affairs to which the managers must adjust their operations in detail. (p. 303)

The market for corporate control thus places strict limits on the ability of managers to pursue their own goals rather than those of the capitalist-entrepreneurs.

The Austrian view of the entrepreneurial market process sheds light on the debate over the effectiveness of the takeover mechanism in providing managerial discipline. A common concern is the belief that financial markets produce too few disciplinary takeovers, due to a free-rider problem associated with tender offers (Scharfstein 1988). Critics point out that if the difference between the current (undervalued) price of the firm and its after-takeover market value is common knowledge, then the target firm's shareholders will refuse to tender their shares until the current price is bid up, appropriating a share of the returns to the acquiring firm. These critics conclude that regulation, not the takeover market, should be used to discipline managers.

The flaw in this argument is that it assumes perfect knowledge by investors. The typical shareholder will *not* usually have the same information as incumbent managers, outside "raiders," and other specialists. It is not in the small shareholder's interest to learn these details; that is why he delegates such responsibilities to the managers in the first place. As Hayek (1945) described it, there is a "division of knowledge" in society. The raider who discovers a difference between a firm's current market value and its potential value under new management has an opportunity for an entrepreneurial profit (less the transaction costs of takeover). Because shareholders have delegated these responsibilities, they will not usually earn a share of this profit. Moreover, the post-takeover market value of the firm is uncertain; the raider's profit, if he is successful, is the reward for bearing this uncertainty. In this sense the takeover artist is a Misesian capitalist-entrepreneur. Successful takeover specialists earn pure entrepreneurial profit; the rest suffer entrepreneurial loss. Raiders thus

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have powerful incentives to undertake disciplinary restructurings, and this places strict limits on managerial discretion.

We should therefore be cautious in attributing the eventual divestiture of many acquisitions, particularly during the 1960s and 1970s, to managerial motives. Moreover, the claim that the acquisitive conglomerates of the 1960s and 1970s were inefficient is inconsistent with recent evidence that during those years, diversifying acquisitions–particularly those that created internal capital markets–tended to increase the market values of the acquiring firms (Matsusaka 1993; Hubbard and Palia 1999; Klein 2001). In light of this evidence, "[t]he simple view that the 1980s 'bust-ups' were a corrective to past managerial excesses is untenable" (Matsusaka 1993, p. 376). In short, both theory and empirical evidence cast doubt on the conventional wisdom that corporate managers made systematic, predictable mistakes by acquiring (often unrelated) subunits during the 1960s and 1970s, and that financial-market participants made systematic mistakes by approving these acquisitions.

Mergers, Sell-offs, and Efficiency: Theory and Evidence

Why, in general, do firms expand and diversify through merger? Why do they sometimes retreat and "refocus" through divestiture? The theory of merger is a subset of the theory of the optimal size and shape of the firm, a relatively undeveloped area in the Austrian literature. Klein (1996) argues for a modified Coasian, or contractual, view of firm boundaries, in which the limits to organization are given by the need to perform economic calculation using prices generated in external markets. Other writers see the Austrian approach as more congenial to the resource-based theory of the firm, defining firms' capabilities in terms of Hayekian tacit knowledge (Langlois 1992, 1994; Minkler 1993). In either case, we can think of merger or takeover as a response to a valuation discrepancy: Acquisition occurs when the value of an existing firm's assets is greater to an outside party than to its current owners. Put differently, merger can be a response to economies of scope, in that the value of the merging firms' assets combined exceeds their joint values separately.[8] As

[8]Two popular explanations for multiproduct economies of scope center on internal capital markets and strategic resources. According to the internal-capital-markets hypothesis, as expressed by Alchian (1969); Williamson (1975); Gertner, Scharfstein, and Stein (1994); and Stein (1997), internal capital markets have advantages where access to external funds is limited. The central office of the diversified firm can use informational advantages, residual control rights, and its ability to intervene selectively in divisional affairs to allocate resources within the firm better than the external capital markets would do if the divisions were stand-alone firms. In the resource-based view, the firm is regarded as a stock of knowledge, establishing a range of competence that may extend across multiple product lines. Excess profits or supranormal returns are seen as rents accruing to unique factors of production (Montgomery and Wernerfelt 1988) and firms diversify because they have excess capacity in these unique factors.

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with any voluntary exchange, the transaction is (ex ante) advantageous to both parties, and should thus be welfare-enhancing.

New combinations of corporate assets can generate efficiencies by replacing poorly performing managers (Jensen and Ruback 1983; Mitchell and Lehn 1990), creating operating synergies (Weston, Chung, and Hoag 1990, pp. 194-95), or establishing internal capital markets (Alchian 1969; Williamson 1975; Gertner, Scharfstein, and Stein 1994; Stein 1997). In particular, considerable evidence suggests that the market for corporate control disciplines incumbent management. For example, Morck, Shleifer, and Vishny (1988) found that firms with lower Tobin's q-ratios are more likely to be targets of takeovers. Tobin's q measures the ratio of the firm's market value to the replacement cost (or book value) of its assets. Because firms with low market-to-book ratios have low expected cash flows relative to the amount of invested capital, the market-to-book ratio can be interpreted as a measure of the firm's investment opportunities (Smith and Watts 1992; Gaver and Gaver 1993), or as a measure of managerial inefficiency or agency conflict within the firm (Lang, Stulz, and Walkling 1991). Low-q firms are the most likely takeover targets.

Given the benefits of takeovers, why are many mergers later "reversed" in a divestiture or spin-off? Here we distinguish between two basic views. The first, which may be termed *empire-building*, holds that entrenched managers make acquisitions, often paying with the acquiring firm's (inflated) stock, primarily to increase their own power, prestige or control. These acquisitions produce negligible efficiency gains, and are thus more likely to be divested ex post. Most important, because the acquiring firm's motives are suspect, such acquisitions are ex ante inefficient; neutral observers can predict, based on pre-merger characteristics, that these mergers are unlikely to be viable over time. By permitting these acquisitions, capital-market participants are also guilty of systematic error. Admittedly, in the empire-building view, markets did eventually correct these mistakes with the restructurings of the 1980s.[9] Still, farsighted regulators could have reduced social costs by limiting such acquisitions in the first place.

A second view, which we term *entrepreneurial market process*, acknowledges that unprofitable acquisitions may be mistakes ex post, but argues that

[9]This raises the question of why, if managers were sufficiently entrenched to make inefficient acquisitions in the first place, would they not remain sufficiently entrenched to hold on to poorly performing targets, rather than divest them and risk revealing their underlying objectives? Boot (1992, p. 1402) argues that an entrenched manager will not divest because the external market will take divestiture as an admission of failure and a bad signal of his ability. The argument that divestitures indicate agency problems thus assumes a change in corporate control between the original acquisitions and the later divestitures.

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poor long-term performance does not indicate ex ante inefficiency. In the market-process perspective, a divestiture of previously acquired assets may mean simply that profit-seeking entrepreneurs have updated their forecasts of future conditions or otherwise learned from experience. As Mises (1966, p. 252) puts it, "the outcome of action is always uncertain. Action is always speculation." Consequently,

> the real entrepreneur is a speculator, a man eager to utilize his opinion about the future structure of the market for business operations promising profits. This specific anticipative understanding of the conditions of the uncertain future *defies any rules and systematization.* (p. 585, emphasis added)[10]

As discussed above, this notion of entrepreneurial decision-making under uncertainty squares with recent theories of acquisitions as a form of experimentation (Mosakowski 1997; Boot, Milbourn, and Thakor 1999; Matsusaka 2001). In these models, profit-seeking entrepreneurs can learn their own capabilities only by trying various combinations of activities, which could include diversifying into new industries. Firms may thus make diversifying acquisitions even if they know these acquisitions are likely to be reversed in a divestiture. This process generates information that is useful for revising entrepreneurial plans, and thus an acquisition strategy may be successful even if individual acquisitions are not. In these cases, the long-term viability of an acquisition may be systematically related to publicly observable, premerger characteristics associated with experimentation, but not characteristics associated with managerial discretion.

To explain the particular pattern of mergers and acquisitions observed, over the last several decades, market-process explanations must appeal to changes in the legal, political, competitive, or regulatory environments that affect the ability of entrepreneurs to anticipate future conditions. Why, for instance, was it particularly difficult for entrepreneurs to forecast the success of acquisitions in the 1960s and 1970s? Why did entrepreneurs feel a greater need to experiment with various combinations of businesses during those years?

One possibility is that complex organizations with active internal capital markets were necessary in the 1960s, but became less important after capital markets were deregulated in the 1970s. The investment community in the 1960s has been described as a small, close-knit group where competition was minimal and peer influence strong (Bernstein 1992). As Bhide (1990, p. 76)

[10]Mises (1949, pp. 110–15) replaces Knight's (1921) distinction between risk and uncertainty with that between "class probability," in which members of a class of events share certain characteristics, and "case probability" in which each event is a unique case.

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puts it, "internal capital markets . . . may well have possessed a significant edge because the external markets were not highly developed. In those days, one's success on Wall Street reportedly depended far more on personal connections than analytical prowess." During that period, the financial markets were relatively poor sources of capital. In 1975, the SEC deregulated brokerage houses and removed its rule on fixed-price commissions. The effect of deregulation, not surprisingly, was to increase competition among providers of investment services. "This competitive process has resulted in a significant increase in the ability of our external capital markets to monitor corporate performance and allocate resources" (p. 77). As the cost of external finance has fallen, firms have tended to rely less on internal finance, and thus the value added from internal-capital-market allocation has fallen. Consequently, firms have adopted simpler, more "focused" structures that rely more heavily on external capital markets and outsourcing, possibly explaining some of the divestitures observed in the last two decades.

Are Divestitures Predictable? Evidence From a Duration Study

This section summarizes our own research (Klein and Klein 2001) on the causes of divestiture. If acquisitions are most often symptoms of managerial empire-building, as suggested by Ravenscraft and Scherer (1987), Porter (1987), and other critics, then pre-merger characteristics associated with high levels of managerial discretion should be systematically related to the long-term failure and reversal of these acquisitions. In the market-process view, by contrast, long-term performance should be correlated only with pre-merger characteristics associated with experimentation, rapidly changing environments, or knowledge-intensive industries. Our empirical research finds little support for the empire-building hypothesis, and much stronger support for the market-process view. Specifically, we find that most characteristics typically associated with empire-building are poor predictors of merger duration. The evidence also suggests that divestiture is more likely when the original acquisition is driven by industry-specific competitive or regulatory shocks.

We studied 222 pairs of firms that merged during the 1977-1983 period. Of the 222 acquisitions, 64, or almost 30 percent, had been divested by July 1995. We used a duration or "hazard" model to study the effects of pre-merger characteristics on the time to divestiture. Duration models help explain how exogenous factors, unobserved factors, and time itself affect the average duration until some discrete event (in our case, divestiture) occurs. Duration analysis allows us to see, historically, how characteristics of the acquiring and acquired firms affect the likelihood that the acquired firm will later be divested.

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To study the effects of pre-merger characteristics on average merger duration, we estimated a hazard regression of duration (measured as the natural logarithm of the number of days) on a constant and on a series of potentially exogenous factors. For assessing the empire-building hypothesis, we included three characteristics associated with high levels of managerial discretion: relatedness of target and acquirer, differences in price-earnings ratios, and the medium of payment. Relatedness addresses the common view that managers deliberately pursue unrelated targets to expand their control or make themselves more valuable to the firm. Following Kaplan and Weisbach (1992), we examined this claim by constructing a dummy variable equal to one if the acquiring and target firm share at least one two-digit SIC code and zero otherwise.

Differences in price-earnings ratios are commonly seen as another indicator of merger motives. Merger critics have often suggested that acquiring firms grow and prosper by "bootstrapping." This refers to the practice whereby bidding firms seek targets with low P/E ratios to boost their reported earnings per share. It is trivially mathematically true that when a firm with a high P/E multiple acquires a firm with a low P/E multiple and pays with its own stock, the acquirer's earnings per share will rise, simply because the combined earnings of the two firms will then be divided by a smaller number of total shares outstanding. Hence, it is argued, acquiring firms can expand rapidly, with market approval, as managers exploit this accounting opportunity.[11]

Of course, this argument assumes market participants could be systematically fooled by a simple algebraic trick. Admittedly, much more complicated financial and balance-sheet manipulations are often used in corporate-control transactions: bidders in the 1960s and 1970s sometimes financed acquisitions with convertible bonds, convertible preferred stocks, and other unique instruments. Although investors could (and eventually did) require that earnings be reported on a "fully diluted" basis, to take account of these manipulations, Malkiel (1990, p. 61) reports that "most investors in the middle 1960s ignored such niceties and were satisfied only to see steadily and rapidly rising earnings." However, there is no evidence that bootstrapping was either prevalent or successful (Barber, Palmer, and Wallace 1995; Matsusaka 1993).

[11]This is Malkiel's (1990) explanation for the conglomerate boom:

> [T]he major impetus for the conglomerate wave of the 1960s was that the acquisition process itself could be made to produce growth in earnings per share. . . . By an easy bit of legerdemain, [conglomerate managers] could put together a group of companies with no basic potential at all and produce steadily rising per-share earnings. (p. 58)

For a more balanced discussion of the bootstrapping practice see Lynch (1971, pp. 55-56).

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Nonetheless, we included a measure of relative P/E ratios to see if acquirers that choose targets with lower P/E ratios are historically more likely to divest those same targets, implying that bootstrapping does tend to fool investors, as increases in reported earnings disguise inefficient acquisitions.

We also included a series of dummy variables to represent the medium of payment used in the merger. Several theories suggest that how an acquisition is financed can affect performance. First, the bootstrapping technique described above works only for mergers financed by stock swap. Second, Jensen (1986) holds that financing takeovers by issuing debt serves to discipline the acquiring firm's management by reducing post-merger discretion in the use of free cash flow. If true, we would expect entrenched managers to avoid making acquisitions using debt, opting for stock swaps instead.

In the market-process or experimentation view, by contrast, divestitures occur when the acquirer receives new information about the target after the merger has taken place. Plausibly, some relevant characteristics of potential target firms can be learned only by experience, forcing entrepreneurs to revise their plans accordingly. What kinds of targets are most likely to have unknown characteristics? Large firms engage in more activities than smaller firms, so potential acquirers have more to learn. On the other hand, smaller firms are less likely to have been written about in the business press, so one could plausibly argue that private information is a bigger problem for small targets.[12] We include target size in our regressions to see if either aspect is relevant. Firms in rapidly changing, knowledge-intensive industries are also likely to have characteristics hidden from potential acquirers. To capture this effect, we created a dummy variable based on two-digit SIC codes. The dummy takes a value of one if the target is in any of the following industries: computers (systems, software, and services), medical products, communications, aerospace, and miscellaneous high-tech industries. We also included target R&D because R&D is difficult to value, especially to outsiders, and thus firms in R&D-intensive industries are likely to have hidden characteristics. Potential acquirers would know potential targets' R&D expenditures (which is reported), but this may not give the acquirers much information on the quality of the research or even the content of the R&D.

Finally, unrelated acquisitions, as discussed above, may be a form of experimentation, as firms try new combinations of activities to find those that best fit their existing capabilities. Although match-seeking acquisitions are more likely to be divested they can still be part of a value-maximizing acquisition strategy. Our relatedness variable, described above, can proxy for match-seeking behavior. However, the best way to identify match-seeking

[12]However, in our sample, all targets are themselves publicly traded corporations, so lack of media exposure is unlikely to be a problem.

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firms is to look directly at historical patterns of acquisitions and divestitures. A firm with a history of repeated acquisitions and divestitures, especially acquisitions into unrelated industries, is likely to be a match-seeker, and thus any current acquisition is more likely to be divested. Unfortunately, our merger sample is too small to compile detailed acquisition and divestiture histories on individual acquirers. As a first approximation, we searched our sample for acquiring firms with at least one previous acquisition that was later divested within a few years of the acquisition. We created a dummy variable equal to one if the acquirer in a particular merger met these criteria, and zero otherwise.[13]

As noted above, periods of intense merger activity in particular industries may be responses to industry-specific competitive or regulatory shocks. Mitchell and Mulherin (1996), Andrade and Stafford (1999), and Andrade, Mitchell, and Stafford (2001) argue that mergers tend to occur in industry clusters, suggesting that industry-specific factors are important. Mitchell and Mulherin argue that corporate takeovers are often the most cost-effective way for industries to respond to these shocks. Moreover, they add, "because takeovers are driven in part by industry shocks, it is not surprising that many firms exhibit volatile performance following takeovers, with actual failures following some negative shocks" (Mitchell and Mulherin 1996, p. 195).

To capture the effects of industry-specific shocks such as regulatory and tax changes, we included measures of industry clustering, both for acquirers and for targets, in our regressions. We did find substantial clustering in our sample. For example, slightly more than half of the mergers during 1977-1983 period occurred in only five of the thirty-seven (two-digit) SIC industries in our sample, both for acquirers and targets, with three-quarters occurring in ten industries. We constructed clustering variables by counting the number of mergers in each two-digit SIC category in each year and creating variables for each merger corresponding to the number of mergers occurring (a) within one year of the merger under observation (including the year before the merger, the year of the merger, and the year after the merger, to span a three-year window), (b) within two years of the merger (a five-year window), and (c) within the entire sample (a seven-year window). If risky acquisitions or increased levels of experimentation tend to appear during periods of industry-specific shocks, then these clustering variables will have negative and significant effects on the length of time to divestiture.

Our results challenge the empire-building hypothesis and offer evidence more consistent with the market-process view. Of the variables associated with

[13]Mosakowski (1997) suggests that younger firms face greater uncertainty, or a higher level of "causal ambiguity," about the best use of their resources, which implies that firm age could also be a proxy for match-seeking behavior.

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agency problems, only relatedness has a statistically significant effect on merger duration. Neither P/E differences nor the medium of payment has a statistically significant coefficient in any of several specifications. The coefficient on relatedness is positive, meaning that related acquisitions are less likely, on average, to be divested (as in Kaplan and Weisbach 1992). However, the coefficient on relatedness is also consistent with the market-process explanation. Of the five variables associated with this view, the coefficient on relatedness is positive and significant, the coefficient on target R&D is negative and significant, and the coefficient on the match-seeking indicator is negative and significant, all as expected. The coefficients on target size and the indicator for high-technology industries have the expected signs (positive and negative, respectively) but are not statistically significant.

Overall, our findings suggest that the divestitures in our sample are not, on average, the predictable result of unwise acquisitions. Rather, divestitures follow experimentation and learning, healthy characteristics of a market economy. Moreover, industry clustering appears to have a regular effect on average merger duration. The coefficients on our acquirer-clustering variables are consistently negative and statistically significant. (The coefficients on the target-clustering variables, by contrast, are not statistically significant.) This suggests that volatile performance does follow shocks, as suggested by Mitchell and Mulherin (1996).

This finding is consistent with the view that firms make acquisitions when faced with increased uncertainty (see Spulber 1992, pp. 557–59). Regulatory interference could be a major cause of such uncertainty. As discussed above, government intervention makes economic calculation more difficult, and can ultimately render calculation impossible. When faced with increased regulatory interference, firms apparently respond by experimenting, making riskier acquisitions, and consequently making more mistakes, ex post.

Conclusions

Do entrepreneurs make predictable mistakes? Theory and evidence suggest otherwise. Contrary to the conventional wisdom on mergers and sell-offs, divestitures of previously acquired assets do not necessarily indicate that the original acquisitions were mistakes. Indeed, empire-building motives do not seem to be systematically related to long-term merger performance. Instead, divestitures are more likely to be associated with experimentation, learning, and other socially beneficial activities.

Acquisitions are uncertain endeavors, and the entrepreneur-promoter—along with the manager to whom he delegates authority—is a speculator. If the consequences of his actions were determinate, he would not be an entrepreneur, but rather, as some economic theories seem to treat him, "a soulless

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automaton" (Mises 1966, p. 585). However, the future can never be known with certainty; long-term profit and loss cannot be predicted based on current information. As Mises explains:

> What distinguishes the successful entrepreneur and promoter from other people is precisely the fact that he does not let himself be guided by what was and is, but arranges his affairs on the ground of his opinion about the future. He sees the past and the present as other people do; but he judges the future in a different way. (p. 585)

As discussed above, it is hardly surprising that these judgments are less than perfect. The relevant question for policy is whether there is a feasible alternative to market-based corporate governance. Our reading of the political-economy literature leaves us doubtful that such an alternative exists.

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Political Union vs. Economic Cooperation

Young Back Choi

The rather sudden unification of Germany in 1990, along with the collapse of socialism in various parts of the world, caused much excitement and urgency about the future of the two Koreas. For a time, estimating the cost of a German-style unification in Korea was a growth industry. But the gradual realization of the costs and difficulties involved in unification had a sobering effect on the initial excitement of a unified Korea. In addition, the 1997 financial crisis in South Korea further dampened the remaining enthusiasm for a German-style unification.

The current South Korean "sunshine policy" reflects the popular sentiment well. The principal aim of the sunshine policy is, not to push for immediate unification, but instead to establish a durable peace based on the coexistence of the two independent countries. The rationale is to avoid a course of events that would prove costly, that is, either a total collapse of the famine-stricken North or a desperate bravado in military ventures. Unification today, in South Korea, is seen only as a long-term goal.

The North Koreans, however, sometimes have behaved unpredictably. Instead of reducing tension on the Korean peninsula and focusing on improving its economy, the North seems intent on escalating the tension—that is, threatening the development of nuclear weapons, provoking a series of naval conflicts, and launching medium-range ballistic missiles, among other provocations. Some of these calculated moves may be initiated to gain bargaining leverage and to increase foreign aid, since North Korea can no longer rely on its erstwhile ideological allies, Russia and China, for bailouts. In the process, the North has succeeded in projecting itself as a menace.

The North's perverse behavior reflects the difficulties facing its leaders: the choice between the urgent need to revitalize the economy through reform on the one hand, and the danger of reform on the other. North Korea's economy

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has literally collapsed, operating at about 20 percent of what it was in the late 1980s. Famine has struck the country. Millions are reported to have died of starvation and millions more are threatened with the same fate. Contributing factors to this disastrous situation include the decades-long adherence to the mistaken policy of *juche* (self-reliance), the failure of central planning, the disappearance of foreign aid from Russia and China, and a devastating string of floods. It should be clear to the North Korean leaders that they have to reform and open up their economy. Yet, reform threatens the very foundation of their totalitarian regime. The power of the North Korean leaders is based exclusively on the cult of personality and the militarized sector of the economy, which is intent on raising income by any means, from exporting arms, mercenaries, and laborers to counterfeiting and trafficking narcotics. Any attempt to reform the economy requires loosening the rigid regimentation and introducing economic incentives, and that threatens their very own power base.

This difficult situation, I believe, is further compounded by the deeply held suspicion by those in the North that the South's ultimate aim is unification (that is, annexation or absorption under any other name), and this further threatens their power base. In fact, the term "sunshine"–adopted from *Aesop's Fables*–could be interpreted as betraying the ultimate intention of the South.

Most public debate on the future of the two Koreas rests on the universally accepted presumption that the two republics will eventually be unified. After all, ever since Korea was arbitrarily divided in 1945 by the superpowers, reunification has been the stated national goal of both. Generations of Koreans on both sides have been raised to believe that to think otherwise is to commit one of the worst treacheries.

Discussions on the future of the two Koreas, consequently, focuses on different scenarios of unification and on how to deal with the associated problems arising from those different scenarios. The estimation of the costs of German-style unification is one area; several others include: issues of monetary union, issues of the legal structure (for the North), questions of property claims, and issues of balanced regional development.

The presumption of (eventual) unification of two Korea entails many difficulties arising from the unique situation of the two Koreas.

It is time to reexamine the idea of unification itself. Unification involves a number of distinct elements–territorial consolidation through political union, customs union, currency union, uniform legal codes, free movement of resources and goods, etc.–each of which entails different benefits, costs, rights, and obligations. The purpose here is to critically analyze the idea of unification by addressing the following questions: What do people mean by unification? What benefits do people expect from it? What would be its consequences?

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Must unification be swallowed whole, or is it possible to derive most of the benefits expected from unification without actually going through unification? I believe that the net benefit of unification is grossly overestimated.

If the unfounded optimism about the gains from unification can be deflated in the South, there is a better chance for peace. This would make the South Koreans more cautious and open to exploring alternatives that would allay the North's fear that the South has a hidden agenda in suing for peace in the peninsula. A fair understanding of the source of the primary benefits of unification will be a sound basis for the exploration of alternatives.

In the remainder of the article, I first examine the prevailing views on the expected *benefits* from unification and find them to be grossly *overestimated.* The most significant benefit would be from the possibility of a greater division of labor and gains from trade, however, this is primarily associated with peace, not necessarily with unification. I then examine the prevailing views on the expected *costs* of unification and find them to be grossly *understated*, ignoring not only the possibility of cost overruns, but the costs associated with the growth of government and the potential for regional conflicts. Considering the most important of the putative benefits of unification can be had without unification, I argue that "no unification" could be a superior alternative for the South. The development prospects of North Korea with and without unification are compared and then it is argued that the North would have better long-term development prospects without unification than with unification. Finally, the possible objections for a no-unification policy because of the anticipated difficulties are not insurmountable, and that no unification should be given a serious consideration as an alternative.

Expected Gains from Unification

According to the prevailing view in the South, there is much to be gained from a unified Korea: (1) acquisition of natural resources; (2) acquisition of cheap labor; (3) enhanced power of a larger political entity; (4) reduced defense spending; and (5) gains from trade. But how realistic are these presumed gains?

Natural resources

Many in South Korea believe that unification would make available valuable natural resources–for example, rich mineral deposits, forests, and hydroelectric potential–now in the North. I believe they have vastly overestimated the value of resources in the North. As for mineral deposits, the current view is a holdover from the Japanese colonial period, when Japan tried to take advantage of mineral deposits and hydroelectric power in their scheme of the "Asia Co-Prosperity Sphere." But things have changed. Today, the mineral deposits in North Korea are not as economically significant as many people

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believe. The secular trends of declining commodity prices in the global market and declining transportation costs have rendered mineral deposits within the national border largely irrelevant. Most of the minerals, even if they are not yet depleted, may be obtained more cheaply in the international market. The potential value of the hydroelectric power is not certain given the availability of low-cost nuclear power. After all, the Korean Peninsula Energy Development Organization (KEDO) is building two nuclear power plants, not hydroelectric dams. As for timber, North Korean mountains reportedly are denuded from overharvesting. A significant gain in natural resources is largely a myth.

Even if there are many valuable natural resources, acquiring them through a political union would be far inferior to acquiring them from the North through trade. The reason is that South Korea would be acquiring through unification, not a no-man's land, but a densely populated poor country—with all the responsibilities it entails.

Low-cost, high-quality labor

It is true that wages are currently low in North Korea. After unification, however, the attractiveness of low wages would evaporate, as South Korea would be forced to provide welfare comparable to what currently exists in the South. After unification, the government could not restrict movement of people within the national boundaries, so North Koreans would move to the South looking for greener pastures, competing for jobs, and seeking welfare benefits. Nor would it be possible to legally discriminate against North Koreans for welfare provisions. Therefore, South Korean labor would be likely, as happened in Germany, to demand a generous provision of welfare in the North, fearing losing jobs to a flood of North Korean labor. Consequently, low wages in the North would become, after unification, a thing of the past.

The judgment of high-quality labor is questionable, as well. If the quality of labor were only judged by years of schooling, one could conclude that the North has a high-quality labor force. But if consideration is given to what the education consists of and what kind of work habits and attitudes people have acquired under socialism, the judgment must be greatly tempered. Much retraining, both formal and informal, would be necessary to convert socialist workers into free workers. However, the quality of labor would not be an issue if the two Koreas remained separate countries, for low enough wages will make any labor attractive.

Enhanced bargaining power

Many also expect that a larger national market would mean greater bargaining power in international trade negotiations. It could be an important consideration for trade negotiators who feel that South Korea has been bullied by bigger economies. However, the North Korean population is around twenty-three million and is currently a subsistence economy on the verge of mass

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starvation, suffering from years of "industrial cannibalization." The increase in purchasing power after unification, therefore, would not be significant, making the prospect of greater bargaining power more imagined than real. The situation could be altered somewhat if the South were to provide a generous transfer of money to the North. The increased purchasing power in the North, however, would come at the expense of the South Koreans. In either case, there would be no significant extension of the market due to unification, at least in the short to medium term.

Some politicians and bureaucrats think that unification would enhance Korea's bargaining power in trade negotiations, but against whom could South Korea use it? Against the United States, the European Union, Japan, or China, each of which has a much larger national market? Given its heavy dependence on trade with these other markets, South Koreans probably have more to lose from playing hardball in trade negotiations.

Reduced defense spending

Currently the two Koreas devote extensive resources in trying to deter the other side from gaining a decisive military advantage. A united Korea would mean drastically reduced spending on national defense. Reduced defense would release for other uses a significant amount of human resources. If the military forces were reduced over time by two-thirds of their present size, more than four hundred thousand young men would be released from military duty each year in South Korea alone! Reduced military spending could lead to a tax reduction of several billion U.S. dollars a year. In addition, a great deal of land could become available for civilian use, as military bases would be closed and inter-Korea combat zones eliminated. For example, the DMZ that has been no-man's land for nearly a half a century would be available for peaceful uses. North Korea's savings would be greater, as it is proportionally more heavily militarized than the South. In short, there would be significant "peace dividends" for both Koreas, or for a unified Korea.

The real question is whether unification is the only way to reduce defense spending. If the two Koreas could come to an agreement on peaceful coexistence, then a similar gain could be had without unification. While the "peace dividends" are real and significant, political union is not necessary; peace on the Korean peninsula would be sufficient.

Gains from trade

By far the most important of the expected benefits of unification is the possibility of free or freer trade within one national boundary and what that would mean for increased productivity from a greater division of labor and from greater scale economies.

Trade, however, follows price differences and profit opportunities. It is not necessary for countries to form a political union to benefit from trade,

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especially not in today's world, where trade barriers have been significantly lowered through multilateral agreements since World War II. Since the 1960s, South Korea has benefited much from increased trade with other countries. The idea that political union is necessary to reap the gains from trade, though such an idea seems to be eagerly pursued in Europe, is an expression of nationalism and mercantilism.

A brief consideration of the anticipated gains from unification reveals the following: (1) there is a great deal of exaggeration in the anticipated gains; (2) the value of natural resources in the North is grossly overstated, especially considering various costly obligations a political union entails; (3) the benefit of cheap labor would disappear after unification; (4) the alleged advantage of gaining a greater bargaining power would likely be more harmful than beneficial; and (5) the most significant expected gains–reduction in military spending and gains from trade–can be had by eliminating the inter-Korean hostility, without political union.

Costs of Political Union

Since the magnitude of the costs of unification would depend on the specific manner of unification, the discussion of the unification costs here dwells necessarily on generalities. Political union of the two Koreas would be extremely costly. The most obvious costs would come from developing the North and providing for the North Koreans during the transition. There would be other costs–perhaps less obvious ones, but not less serious–namely, the almost certain expansion of government and the potential social conflicts. Let us consider them in turn.

Costs of development and welfare provisions

The North Korean economy has nearly disintegrated and is faced with major famine. The North blames bad weather for the famine. But the real reason for the current state of affairs is the decades-long pursuit of central planning combined with the doctrine of *juche* (self-reliance), resulting in economic backwardness and severe industrial cannibalization. In the 1960s, the North boasted successful industrialization in the fashion of Communist Russia. However, after the potential of the forced mobilization had been exhausted–along with the patience of ideological allies in avuncular Russia and China in supplying the North with aid–the North Korean economy steadily declined for nearly two decades. If the Korean peninsula were to be unified as a political unit, the South basically would take over an area larger than itself, populated by nearly twenty-three million destitute people, without much industry, or resources. (The only valuable acquisition would be the one million-man North Korean army, whose loyalty would remain uncertain for some time.)

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The costs of developing the North would be staggering, if the South were to bankroll it. Taking a broad view, costs are of the following kinds: costs of dismantling, restructuring, and privatizing uneconomical state enterprises and other assets; costs of building up infrastructure; costs of cleaning up severe environmental degradation; costs of assuming North Korea's external debts (largely to Russia, China, and Japan); and costs of providing welfare for the North Koreans during the transition. In addition, there would be costs associated with settling the property disputes and claims by millions of North Korean refugees (and their heirs) in the South and elsewhere. In light of the German experience, massive cost overruns must be expected. (Through 1998, the total amount of West German subsidies to East Germany came to $560 billion. In 1999, there was an additional $22 billion.)

Therefore the "peace dividends," mainly from reduced defense spending, would fall far short of the required expenditure. The gap, then, would have to be filled either by government borrowing, or printing money, or by additional taxes. Financing the development costs would not only impose burden on the South Koreans, but would also introduce much distortion in the economy. The "peace dividends," however, need not be squandered (and additional burden assumed) if peace on the peninsula is achieved without political union.

Alienation and social conflict

Unification is likely to breed mutual resentments between the North and the South. As it stands now, South Koreans most likely would take charge—they would do the financing and assume the management. Though the majority of North Koreans would benefit from the massive money transfer from the South, they still might resent South Korean domination in all areas, that is, finance, business, politics, academics, etc. South Koreans, in turn, would likely be frustrated with the slow progress and resent the heavy burdens they would have to shoulder, especially if North Koreans showed resentment instead of gratitude, and seemed to be content to get by on welfare provisions instead of working. (Indeed, such seems to be the prevalent sentiment between West Germans and East Germans since unification.) There would be other potential sources of resentment as well. For example, if North Koreans were given certain property claims for assets as a part of the privatization process, a large part of the assets would likely be accumulated by South Korean businesses. Then, the South would be blamed for having schemed to defraud the people in the North.

Koreans are not the most tolerant people. Strong regional prejudice and resentment already exist between the Kyungsang province and the Cholla province. The regional conflicts have become highly politicized since the 1960s and threaten national unity within South Korea itself. Regional resentment and prejudice between the North and South could be much worse under unification than anything we have yet seen. This raises the possibility of renegade

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military commanders in the Northern army becoming warlords, demanding tribute and ransom whenever it was opportune to do so.

Growth of government

Seldom considered costs of political union stem from the assured growth of the South Korean government. Many may not view it as a problem, but it is a serious one. After the 1997 financial crisis in the South, many have diagnosed the overbearing government as the root of the evil. Many South Koreans have come to believe that their centralized and bloated government is more harmful than helpful. Yet, they have not been able to roll back the government. Two of the chief difficulties of reducing the role of government in the economy have been the politicians' tendency to look for popular solutions and the bureaucratic reluctance to let go of their "iron bowls." Unification would be a god-sent opportunity for the bureaucrats to expand government under the pretenses of feeding and developing the North. Not only would the government grow bigger, but it would become even more intrusive than now as it would strive to meet certain arbitrarily set targets for development. Moreover, the elimination of a rival regime would remove a check, however meager, on the South Korean government. A further expansion of government in Korea would be a definite negative in terms of personal freedom and economic efficiency.

Based on the above considerations, it seems that common estimates of the costs of unification are vastly understated. When all the costs are fully considered, it is not at all clear whether there would be much, if any, net gain from political union; thus, the argument for political union loses much of its rationale. Even so, the sentimental appeals of unification will tempt many people to insist that there is no alternative. At this point, I would like to suggest that we think about the unthinkable—no unification—and see whether it might be a credible alternative.

No Unification as an Alternative

By no unification, I propose ruling out any thought of ever merging the two Koreas into one nation, as there is no thought of merging Korea with Russia, China, Japan, or the United States, for example. No unification, therefore, means South Korea should regard North Korea as a foreign state, and vice versa—in the manner that South Korea regards the Philippines, or Japan, or Thailand.

No unification is a better alternative because it promises a greater net gain than unification; no unification also is possibly more conducive to peace. This needs some explanation. One, as the above consideration indicates, the nature of expected benefits from unification—the "peace dividends" and the gains from trade—can be had by securing peace. Unification is not a prerequisite for them. Indeed, it was not necessary for South Korea to become a part of a larger political entity to derive much benefit from trading beyond its national boundary. It is obvious that "peace dividends" can be had without

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political union. Other expected benefits, more directly tied to unification, for example, natural resources, greater bargaining power, etc., are grossly exaggerated or elusive. Therefore, there would not be much difference in expected gains with unification and without unification. The main difference would be the emotional appeal of unification. Two, unification would be much more costly than no unification. As noted above, the costs of unification would be much greater than commonly estimated; no unification would mean that South Koreans would not incur many of the costs associated with political union. With no unification, the North would be in charge of its own destiny. There is no need for the South to shoulder most of the costs of transforming the North.

The argument that no unification promises greater net gain than unification does not rest on the precise estimation of the relevant costs and benefits. If that were so, the argument advanced here would be very weak. Instead, what is argued here is: The net gains from unification (U) = gains from unification (G) - costs of unification (C). The net gains from no unification (NU) = gains from no unification (G′) - costs of no unification (C′). G = G′ and C > C′. Therefore, NU > U.

No unification is possibly more conducive to peace. A clear statement of no unification would render the rulers of the North less suspicious about the South's intentions. There would be fewer worries for the North Korean policymakers about the South undermining their power or being dominated in the reform process. Being less suspicious of the South, the North would be more likely to pay attention to the problem of putting its house in order and developing its economy. The North would soon realize, hopefully, that a lasting peace on the peninsula suits their purpose (of staying in power) more than erratic extortionist tactics, from which net gain is small and diminishing marginal return sets in rapidly.

No unification should be seriously considered as a superior alternative to unification from the South's point of view. No unification as a policy would not succeed unless the North also deems it as a superior alternative. If the rulers of the North are concerned with long-term survival, they should certainly consider the development prospect with no unification.

Development Prospects for the North

What is good for the South is not necessarily bad for the North. No unification promises a better development prospect for the North. In order to appreciate this thesis, we must compare the development prospect with unification and the development prospect with no unification.

Development of the North with unification

The development of the North after unification would be characterized by the imposition of the South Korean system and government-directed

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development programs. Much waste, inefficiency, corruption, and social conflict could develop. Given the vast differences in resources under command between the two Koreas, unification would mean that the South Koreans would have to take upon themselves the responsibility of integrating the North and the South into a nation. In carrying out the task, the South Korean system would be imposed on the North.

This raises the question of whether the current South Korean system is most suitable for the development of the North, which faces very different conditions. South Koreans have firsthand knowledge of the difficulties of adopting institutions evolved elsewhere. Moreover, the South Korean system itself is not without its problems. Various problems with the South Korean system have been pointed out especially in the aftermath of the 1997 financial crisis, including excessive government regulations, corruption, weak rule of law, and the absence of civil society. Currently, the situation is much worse in the North, but why should the North be limited to the adoption of the South Korean system, when it could learn from the experiences of developmental and transitional economies and experiment with various possibilities?

There are plenty of examples of successes and failures from which to draw lessons—not only in South Korea, but also in Japan, China, Russia, Vietnam, Taiwan, Cuba, Romania, Albania, etc. The imposition of the South Korean system would needlessly restrict the North's opportunity to experiment with suitable institutions. The North should dread the imposition of the South Korean system, not only for political reasons, but for economic reasons as well.

Government-promoted development

The efficacy of government-directed development efforts is questionable. Massive foreign aid since the end of World War II has seldom succeeded in bringing about meaningful economic development for the recipient. Rather, foreign aid tends to be counterproductive, merely proliferating rent-seeking groups, breeding corruption and inefficiency, and making economies dependent on a constant (or even increasing) infusion of aid. There is little doubt that, given the chance, North Koreans would excel in the rent-seeking game and would do their utmost to frustrate the development effort. Various attempts at internal development have not fared much better. They merely tend to create the growth of government, massive waste, and regional dependency. For all its efforts, for example, the South Korean government has not succeeded in bringing about a balanced regional development within South Korea itself. Can we reasonably expect it to succeed in bringing about balanced growth to the North?

If anything, there is the danger of transforming the program to develop the North into "corporate welfare" for South Korean business interests. Given the state of the economy in the North and the South Korean government's

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willingness to foot the bill in the event of unification, there would be a great demand for capital goods—much not based on profit calculations. Many South Korean producers would regard the North as a captive market for investment goods. Therefore, the unification process would likely be a prime opportunity for South Korean producers of capital goods. But this would distort the direction of economic development in the North, raise the costs of developing the North, and certainly breed more corruption. Moreover, the "corporate welfare" would lead to friction with other countries that covet the market.

Some in the South may object to this reasoning, arguing that the *dirigiste* South Korea has successfully developed its economy and, therefore, has the expertise to develop the North. Indeed, there has been a certain amount of expertise on economic development and policymaking accumulated in the South. However, there is a problem in applying the expertise because the situation is different now. The kind of growth-at-any-cost practices that propelled the South along the path of rapid growth in the 1960s, 1970s, and even 1980s cannot be carried out effectively under the current South Korean system, in which the government can no longer exact near-total submission from its people. Ironically, what expertise South Korea could offer based on its own developmental experience might be put into practice more easily if the North stayed separate.

Development of the North without unification

What is important with a no-unification policy is to convince North Korea that it must proceed pretty much on its own and not count on substantial economic transfer, beyond minimal food relief. Once the principle of no unification is firmly established, North Korea would basically be on its own. The development of North Korea with no unification would then be characterized as self-reliance and self-determination—not in the sense of the misguided policy of *juche* (self-sufficiency), but in the sense that its future would depend largely on its own actions.

Given its dire economic situation, the North would try, as it is presently setting the stage to do, to solicit a package of aid from other countries and international organizations, in a combination of famine relief and other economic aid. But any aid would be different from what the Russians or the Chinese used to provide, or, for that matter, what the United States and South Korea currently provide. Presumably, everyone has learned a lesson or two from the futile attempts of showering LDCs (least developing countries) with aid packages. The foreign aid would come with more strings attached—requiring the North to behave in certain ways that might be contrary to the wishes of the North Korean rulers. Moreover, the aid would be grossly inadequate for the development of the North Korean economy—given that the economy has largely degenerated into something less than a subsistence economy. The North

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Korean rulers would, therefore, have to make up the shortfall in revenue to retain their power. They would face broadly two choices. The first would be to maintain the status quo. This choice is the traditional extortionist approach of exacting additional transfers from grudging donors–in addition to peddling arms, drugs, counterfeits, and contraband. The North used to receive substantial transfers at various times from Russia and China by playing them against each other, exploiting its geopolitical position. Now, the transfers from the erstwhile ideological allies have been either cut off or reduced to a trickle, compounding its difficulties. Recently, the North has tried to cultivate new donors in the United States, Japan, and South Korea by becoming a nuisance. But this tactic is nearsighted–only getting barely enough resources for the rulers to stay in power, and doing little or nothing to improve the lot of the already struggling people. It is a vicious circle and cannot be sustained for long. Either the population would be drastically reduced and, God forbid, soon there would be only a few left to rule, or the rulers might be ousted in a riot or a coup–as in Romania or Albania or Uganda. The new ruling group then would face the same choices again.

The second choice would be reform. The realization that there is no place to fall back on will, sooner or later, lead to reform, if only because the rulers of the North would need to generate revenue to govern and retain their power. The North's proximity to more prosperous neighbors–e.g., Japan, China, and South Korea–not only should suggest to them what to do and what to expect from a serious reform, but also should provide ample opportunity to gain from trade with them. The reforms would not be textbook pretty, but the rulers of the North would do whatever it takes to earn enough to stay in power. Surely, the rulers would maintain a degree of stability for the sake of retaining power. They would also have to experiment with various schemes to generate needed revenue. Such schemes would not succeed in the long run unless a significant portion of the people also benefitted and conformed to the market. The requirement to earn a profit, therefore, would induce the North Korean economy to gradually become (more) market-conforming. The potential is tremendous if only because the North Korean economy has so long been suppressed and distorted. That would be the beginning of a sustainable developmental path–a much better prospect than the one under unification. Over time, North Korea could turn into a rapidly growing economy. Other countries, including South Korea, have chosen this course in the past–only when all other options seemed to have been exhausted–and have done well. There is no reason why the North would not be able to enjoy a similar success. The North Korean leaders would not only have a better chance of retaining power, but a good chance of getting credit for rebuilding the nation and bettering the lot of the people.

Of the two development prospects–unification or no unification–the latter is more attractive. It is not because the North would be guaranteed to grow

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faster or develop better on its own—no one can guarantee such—but that the North would have a chance to do better on its own, if it tried. An independent and prosperous North Korea would then be a good trading partner in economics and a friendly rival in politics to South Korea—as between the United States and Canada, or between South Korea and Taiwan.

Difficulties of No Unification

No unification would not be without its own difficulties, however. They would include: (1) the political viability of advocating no unification; (2) the moral dilemma of tolerating a dictatorial developmental state in the North; and (3) potential refugee problems. The relevant question is whether these are severe enough to render no unification an unattractive option.

Political viability

Introducing the idea of no unification may involve certain political risks. Many South Koreans with a strong sentimental attachment to unification may react negatively toward it. But would people really want unification, at any cost? The enthusiasm for unification has already moderated considerably in light of the high cost of the German-style unification and the sobering experience of the 1997 financial crisis. The paramount issue is how to convince people of the real and stupendous costs of indulging in their gut feelings and of the existence of a far better alternative. How to do it is in the realm of political leadership. It does not seem impossible, however.

Moral dilemma

Once the principle of no unification is accepted, and the North tries to embark on a course of development, the conditions in the North may not be pretty to witness—not by U.S. standards, and not even by the South Korean standards. There may be continued political repression, and the working conditions of laborers could be sub-par for a long time. Some in the South may raise the question: How can we let fellow Koreans in the North suffer under ruthless dictatorship and severe economic hardship? Indeed, the conditions in the North will be difficult to ignore. The correct way to view the situation might be: (1) Do not forget the premise that North Korea is a foreign country; (2) Citizens in South Korea should not expect the government to intervene in North Korean affairs, just as it is prudent not to intervene to rectify conditions in Indonesia or Iraq, for example; and (3) Anyone who might feel morally compelled to aid anyone or any group in the North should be free to do so, as an individual or in collaboration with other like-minded individuals. Many religious people in the South and overseas will be eager to lend a helping hand. One intriguing possibility is organizing volunteers out of many able-bodied people in their late forties and fifties who are in retirement in South Korea. There are many other possibilities.

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Refugees

Reforms in North Korea would mean gradually less regimented life and greater movement of people. Since the conditions in the North would not improve overnight, many would be tempted to emigrate wherever the wage level was substantially higher than that prevailing in the North. The South would be especially attractive because the common language and close proximity would make it easier to settle in and adapt. There is the potential for a mass migration to the South, possibly resulting in a large-scale disruption. Few countries with any moral scruples can effectively prevent people seeking a better life from crossing their borders. What to do? A rapid improvement in economic conditions in the North could, to an extent, reduce the incentive to migrate. While the potential is there, unfortunately there is no known method of engineering a quick economic growth. The South should not even attempt to create quick growth; it would result in wasting resources, inducing misallocation of resources, and retarding its development.

A partial solution lies within South Korea itself–a set of reforms in the South. Especially important are a reduction in welfare provisions and a deregulation of the labor market. If various welfare measures in the South were drastically reduced, the South would become much less attractive to settle in, and to that extent, the migration of people from the North would be reduced. But given the vast differences in the prevailing wages, there would still be a large-scale migration. This is why the labor market must be reformed to be more flexible. If the labor market is made more flexible and if there is little welfare provision, then the influx of people is not necessarily a bad thing. Newcomers would then earn their keep and contribute more to the South Korean economy. The South, therefore, should reform its laws to make the labor market more versatile. With appropriate reforms in the South, refugees might be even welcomed!

Concluding Remarks

The main argument here has been that, for South Koreans, no unification could be an option superior to unification because it promises much greater net gains. This could be true for North Koreans as well. Yet, hardly anyone, anywhere, seems to entertain no unification as a serious option. The reason is that Koreans on both sides have for so long taken for granted the eventual unification of two the Koreas. To think otherwise would seem too disloyal even to contemplate. If the analysis presented here, however, is even half-correct, the option of no unification merits serious consideration, for according to the analyses presented above, unification would be too costly. Moreover, a clear articulation of the principle of no unification could allay suspicion on the part of the North Korean leaders regarding the true motive of the South. In the

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comforting thought of the greater security of their own survival, then, the North Korean leaders might be more willing to cooperate to promote peace in the peninsula, from which everyone would benefit.

It goes without saying that until a lasting peace in the peninsula is attained, the South, as any country faced with another hostile and belligerent country, should maintain a credible defense capability against any external threat. A combination of a clear and formal renunciation (after building a consensus in the South) of the goal of unification—even in the long run—and a strong defense posture should soon convince the North of the extravagant foolishness of escalating tensions in the peninsula, which it could ill afford.

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When you burn the candle at both ends, it goes out too soon

Sleep deprived Americans have a 70% higher death rate. A lack of sleep sets you up for lots of potential problems. Marital strife. Poor job performance. And more. California Department of Health statistics show that people who get less than the recommended amount of sleep have a 70% higher death rate.*

Highways are a particularly dangerous place. Sleep problems put drivers at risk for what experts call *microsleeps*. Uncontrollable waves of fatigue where you nod off at the wheel. Each year drowsy drivers cause at least 100,000 crashes, killing more than 1,500 people and injuring another 71,000.**

If you experience any of these symptoms, you may have a sleep problem.

- ☐ **Snore** loudly.
- ☐ You or others have observed that you **stop breathing** or **gasp for breath** during sleep.
- ☐ **Feel sleepy or doze off** while watching TV, reading, driving or engaged in daily activities.
- ☐ **Have difficulty sleeping 3 nights a week or more** (e.g. trouble falling asleep, wake frequently during the night, wake too early and cannot get back to sleep or wake unrefreshed).
- ☐ Feel *unpleasant, tingling, creepy* feelings or **nervousness in your legs** when trying to sleep.
- ☐ **Interruptions to your sleep** (e.g. nighttime heartburn, bad dreams, pain, discomfort, noise, sleep difficulties of family members, light or temperature).

Sleep help is available. New treatments are helping millions of Americans get the restorative sleep their bodies crave. Now they're rested, happier and more productive, too. Burning the candle at both ends is not only dangerous ... *it's unnecessary.*

Talk to your doctor or go to our web site for more information.

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W W W . S L E E P F O U N D A T I O N . O R G

* 70% higher mortality rate in a 9 year follow-up period for people getting 6 hours of sleep or less per night. Wingard, D.L., Berkman, L.F., Mortality risk associated with sleeping pattern, Sleep, 1983; 6(2): 102-7.
** National Highway Traffic Safety Administration, 1995.

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KALDOR-HICKS EFFICIENCY AND THE PROBLEM OF CENTRAL PLANNING

EDWARD STRINGHAM

Kaldor-Hicks efficiency is widely accepted by the economics profession and is gaining ground in the area of law. Modern law and economics is almost entirely based on Kaldor-Hicks, and judges such as Richard Posner advocate using the standard to decide cases (Posner 2000). Bryan Caplan (1999) defends Kaldor-Hicks efficiency and argues against Austrian economists who reject this neoclassical welfare theory. Caplan (p. 835) states,

> [T]his criterion of efficiency has many advantages over Rothbard's approach. In particular, it actually allows one to make efficiency judgments about the real world–to judge, for example, that Communism was inefficient or rent control is inefficient or piracy was inefficient.

Supporters of Kaldor-Hicks believe it useful to have a quantitative measure to assess the efficiency of different situations.

Although it may appear convenient to be able to judge policies using such an efficiency standard, I will argue that no such measures can be constructed. Contrary to Caplan (p. 834) who states, "Rothbard's own theory strips him of the ability to call any act of government inefficient," I will argue that the neoclassical standard itself cannot be used to judge different states of the world. As Gerald O'Driscoll (1980, p. 359) wrote, Kaldor-Hicks advocates such as Posner are "actually grappling with the calculation problem." This article will follow up on this point and discuss whether Kaldor-Hicks-efficient policies can be calculated. There are a number of criticisms of the wealth-maximization standard on both positive and normative grounds.[1] The focus here

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[1]Chipman and Moore (1978) and Sen (1979) survey the criticisms of new welfare economics, including Scitovsky's (1941) critique that says implementing a policy that is

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will be on the positive pitfalls of Kaldor-Hicks efficiency to argue that it is an unusable standard.

Measuring Kaldor-Hicks Efficiency

To review, the approach pioneered by Kaldor (1939) and Hicks (1939) judges outcomes efficient when monetary wealth is maximized. Posner (1980b, p. 243) explains, "Resources are allocated efficiently in a system of wealth maximization when there is no reallocation that would increase the wealth of society." This is, in Posner's words (1980a, p. 491), "the Kaldor-Hicks or wealth-maximization approach." Kaldor-Hicks efficiency does not measure utility or happiness; it solely looks at willingness to pay in dollar terms. By measuring effects in dollars, it avoids the problems of other traditional welfare economics because it does not rest upon the implausible assumption that different individuals' utilities are measurable and comparable (Robbins 1952).

The key requirement for conducting Kaldor-Hicks analysis is the ability to gather and sum up people's willingness to pay attached to different outcomes. Since we are dealing with the common unit of dollars, this task may appear tractable, but it is a lot easier said than done. For even in the most basic cases, it is not clear how to find people's willingness to pay. One method used is to look at market prices, but the problem with using observed prices is that they merely represent prices from times gone by. Ludwig von Mises (1998, p. 327) noted:

> All the prices we know are past prices. They are facts of economic history. . . . The experience of economic history never tells us more than that at a definite date and a definite place two parties A and B traded a definite quantity of the commodity *a* against a definite number of units of the money *p*.

Merely observing a trade at a given price does not enable us to extrapolate that this price will be applicable to the future.

We know that a trade took place at a given price and we know the price was desirable for both parties at the moment of trade, but we cannot infer that this price will be desirable forever for trading partners. A past price represents how much the buyer was willing to pay only in the moments leading up to the transaction and nothing more. "Cost is that which the decision-taker sacrifices or

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efficient from today's perspective may be inefficient from tomorrow's perspective (once changes that endowments, and relative prices brought about by the new policy are taken into account), which in the end shows the incommensurability of efficiency levels. For the sake of argument, this paper will ignore the Scitovsky double-switching problem and focus on judging efficiency levels using today's viewpoint; it will be argued that even under the simplest conditions, the problems of Kaldor-Hicks efficiency are still intractable. For some of the normative problems, see Block (1977, 1995, 1996, 2000), Hoppe (1993), and Rothbard (1974).

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gives up when he makes a choice" (Buchanan 1999, p. 41). Seconds after a transaction, the buyer, who has satiated his demand at that price, will be unwilling to pay the same price. From that point forward, the buyer has a different amount of money and goods, and will have a different willingness to pay.

The world is constantly changing, so it would not make sense to base decisions on prices that no longer have relevance. Differing market conditions will lead to different market clearing prices. Boudreaux, Meiners, and Zywicki (1999, p. 783) point out, "Prices are not static and absolute. . . . There is no 'willingness to pay' for IBM stock; there is today's price for IBM stock." It would be a folly to take a snapshot of prices and think that we can continually make inferences from them (Salerno 1990, pp. 41-44). The fact that prices vary so much shows that the valuations change according to market conditions.

Even if exogenous variables were fairly constant, there is no reason toexpect to see stable preferences over time. The rank ordering of goods by individuals can change from day to day. Murray Rothbard (1956, pp. 228-29) remarked:

> The prime error here is the assumption that the preference scale remains constant over time. There is no reason whatsoever for making any such assumption. All we can say is that an action, at a specific point in time, reveals part of a man's preference scale at that time. There is no warrant for assuming that it remains constant from one point of time to another.

It would be in error to assume that just because people acted one way in the past that they would prefer to act that same way in the future.

Implementing Kaldor-Hicks Efficiency

Market volatility and changing preferences certainly complicate matters, but even if there were no exogenous changes and constancy of preferences, prices would not provide the necessary information. Prices tell us about exchanges that people agreed to, but Kaldor-Hicks analysis requires us to consider hypothetical situations that have not taken place. Posner (1979, p. 130) believes that "[v]oluntariness is, however, too restrictive a condition and once the domain of the wealth-maximization criterion is expanded to include hypothetical markets, a problem of measurement arises." There may be interventions in the market that increase net wealth, so it is necessary for the government to find out when this would be the case. A policy of laissez-faire would not maximize wealth as much as the policy that approximates laissez-faire but intervenes when intervening increases wealth.[2]

[2]This is analogous to the point made by Smart (1973, pp. 9-12), who said rules that maximize utility are always ones that allow exceptions in the cases when violations increase utility; thus rule-utilitarianism collapses into act-utilitarianism.

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Advocates of Kaldor-Hicks wealth maximization maintain government should take action when there is a positive net willingness attached to an outcome.[3] Posner (1980b, p. 244) explains, "Under the Kaldor-Hicks definition of efficiency . . . a reallocation of resources is efficient if it enables the gainers to compensate the losers, whether or not they actually do so. This is equivalent to wealth maximization." It is important to emphasize that the losers do not need to be compensated; the criterion is simply whether the winners' gains exceed the losers' losses as measured in dollars. For example, if teetotalers are willing to pay a billion dollars to have alcohol outlawed and the willingness of drinkers to pay in order to be allowed to drink is less than a billion, then the efficient policy would be prohibition. Likewise, if a rich person is willing to pay a million dollars to kill a penniless being, allowing the killing is efficient since the willingness to pay attached to that outcome exceeds that of allowing the poor person to live (Block 1996).[4]

Kaldor-Hicks requires judges who are adding up how much each party is willing to pay for a given outcome to determine what is efficient. For Posner, this task is within the capability of the government. Posner (1983, p. 62) writes:

> The purist would insist that the relevant values are unknowable since they have not been revealed in an actual market transaction, but I believe that in many cases a court can make a reasonably accurate guess as to the allocation of resources that would maximize wealth.

Judges can estimate people's willingness to pay and add these values up to make their decisions, but the important part is, Posner does not say how they will do so. As O'Driscoll (1980, p. 357) pointed out, "Judges are human agents, not ideal observers; they do not possess superior knowledge of the future, or of the operation of physical or economic laws." Is it reasonable to

[3]To clarify, it is possible to use Kaldor-Hicks analysis but not support the implementation of Kaldor-Hicks policies. For example, Caplan (1999, p. 834) believes, "Many, if not most, economists would agree with Rothbard that efficiency alone is an inadequate normative criterion." One may debate whether many economists agree with Rothbard (1997b,c) or whether it is constructive to use Kaldor-Hicks analysis, but Caplan's point of differentiating positive and normative uses of efficiency must be granted.

[4]This would be the case if transaction costs of negotiating are prohibitive or if we use Hicks efficiency, which technically is different from Kaldor efficiency. Matthew Adler and Eric Posner (2000, p. 1108) explain a project as, "Hicks efficient relative to the status quo if there is no hypothetical lump-sum redistribution in the status quo world, from project losers to project winners, such that this amended status quo is Pareto efficient relative to the project." In other words, a policy is efficient if the potential loser cannot offer the potential winner enough money that the winner prefers being paid over having the policy implemented and the potential loser prefers paying over having the policy implemented. Cowen (1998) brings up a plethora of problems for cost-benefit analysis, including the fact that willingness to pay and willingness to be paid can differ greatly. In the case of killing, a penniless person might not agree to be killed for billions of dollars, but since he has no money, his willingness to pay not to be killed is zero.

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expect that judges can make reasonably accurate guesses to maximize wealth?[5]

The problem is that in the absence of trade, it is not apparent how an outside observer can know to what degree people value an outcome. Steven Littlechild (1978, p. 85) elaborates:

> In the case where no contract is made, we are considering a hypothetical choice and asking how attractive a hypothetical alternative would need to have been in order to be preferred (or how unattractive it could have been and still have been preferred).
>
> The first point to emphasize is that the proposed procedure does not involve choices, albeit hypothetical, by the person in question but rather by an outside observing economist, government official, judge, or politician.

Government officials would need to guess, not how they would value different choices, but how other people would value different choices. This is easier said than done; Rothbard (1956, p. 225) argued the only way an observer can know if someone values a good is by seeing him actually paying for it. Choice demonstrates preference, and without it, it is not possible to tell how much a person values a good. Judges in this situation are faced with making decisions based on costs and benefits that are unknowable. As Cordato (1992, p. 95) stated, "The arguments that were made in opposition to the Pigouvian tax assessor are equally applicable to the Coasian judge."

The problem is even more complicated than just figuring out the willingness to pay for the two parties involved in an exchange; to determine net willingness to pay the government needs to count the willingness to pay of every third party. Mario Rizzo (1980b, p. 641) pointed out, "In fact partial efficiency is insufficient as a basis for constructing any persuasive normative argument. If, for example, a liability rule is efficient as between two potential litigant-classes, it can be inefficient once third-party or spillover effects are taken into account." For any given decision the government would need to determine the general equilibrium solution to take into account the affects on all people.

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[5]For the sake of argument, it is assumed that judges would actually act with good intentions, but clearly this need not be the case. Theorists such as Prendergast (1993) point to potential problems of employees not working in the interest of their employers, with companies being unable to monitor their performance and failing to maximize the value of the firm, but it must be realized that these problems would exist and may be greatly exacerbated in the judiciary. The private sector has devised many contracts to control performance problems (Brickley, Smith, and Zimmerman 1997) and ultimately has profits and losses that act to set limits (Mises 1990). In the public sector, on the other hand, these feedback mechanisms are all but absent, so there is little reason to expect that judges would do anything other than act in their own self-interest (Hasnas 1995; Stringham 1999). When the public-interested-judge assumption is dropped, Kaldor-Hicks becomes even less reliable.

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Evaluating All Possible States of the World

Measuring the effects on every single person for one outcome is difficult enough, but matters get exponentially more complicated when we realize that we must consider the effects on all people for all potential outcomes. To accurately determine efficient policies, the government would need to figure out each person's willingness to pay under all possible states of the world. Boudreaux, Meiners, and Zywicki (1999, p. 791) write:

> Asking people to reckon their demand curves for all goods, services, and amenities under a welter of different conditions is to ask the impossible. . . . The impossibility of mapping a full schedule of preferences for every given survey respondent means that, by necessity, a certain number of alternatives must be excluded from the menu of options over which a person can hypothetically spend his or her money.

It is unclear how to figure out the willingness to pay for even one person under a myriad of different circumstances, and it is even less clear how to figure this out on a society-wide level.

The heart of the problem is that willingness to pay cannot be detached from actual circumstances of the market. One's demand is a function of existing policies, since existing policies affect endowments and relative prices. Tyler Cowen (1993, p. 254-55) notes how policy affects preferences:

> Endogenous preferences create problems for welfare economics when preferences (or metapreferences) are determined by the policies being chosen or evaluated. . . . Policy options can no longer be ranked unambiguously, because rankings depend upon the distribution of wealth, which in turn depends on policy.

Policies shape the world by determining who is in possession of resources, and since individuals differ, we would expect to see alternate demands depending on how property rights are assigned. If policy is altered, the willingness to pay for all goods is altered, so it would be a mistake to only look at the immediate effects of a policy.

The courts would need to know ahead of time all possible results of each policy and how they would interact with each other. Rizzo (1985, p. 873) elaborates, "There are also substantial interactive effects among decisions and rules that are often impossible to discern. Rules must therefore be applied in particular cases regardless of the hypothesized or 'guessed-at' consequences." The interconnectedness of the economy and uncertainty of the future will cause policies to have consequences that cannot be predicted. Without a crystal ball, it is unlikely that judges will be able to know whether a policy will be wealth-maximizing.

A related problem was pointed out by Herbert Spencer (1978, p. 198), when he criticized Bentham: When all underlying policies are up in the air, it

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is unclear how to measure different states of welfare. He wrote, "For justice, or equity, or equalness, is concerned exclusively with quantity under stated conditions; whereas happiness is concerned with both quantity and quality under conditions not stated." Spencer believed that justice was easy to discern while happiness was not. There are parallels between maximization of happiness and maximization of wealth, since in both cases a nearly infinite number of different possible states of the world would need to be considered.

Without stated conditions, it is difficult to even know where to begin.[6] Posner (1979, p. 108) admitted this problem when he wrote, "In cases like this, where wealth effects are important, economic analysis does not predict a unique allocation of resources unless the initial assignment of rights is specified." One option would be to assign rights based on some notion of justice but, according to what is valued from today's viewpoint, this may be Kaldor-Hicks inefficient.

To truly advocate Kaldor-Hicks efficiency, all rights need to be assigned in a way that maximizes wealth. In an uncertain world, however, it is impossible to know which assignment of rights will produce the wealth-maximizing results. Even small policy changes can have large affects on society that cannot be predicted.[7] Posner (1983, p. 77) himself brings up an example that can be used to illustrate this point. He hypothesized, "We may be reasonably confident that the American people would be poorer if Henry Ford had decided to become a Trappist monk rather than an automobile manufacturer." If this is the case, it is imperative to wealth maximization that other would-be-Henry Fords are prevented from entering the wrong professions. Talented young people would need to be directed into the industry where they would produce the greatest impact on wealth.[8]

It may be true that the world would be a lot less rich if Henry Ford had not worked in the automobile industry, but who would have known that ahead of time? If Henry Ford had focused his efforts on religion, it is possible that he would have come to an important revelation that would increase wealth by more than he produced in the automobile industry. It is impossible to compare things to how they might have been ex post, and it is impossible to predict how things will be ex ante.

To determine the set of policies that will be wealth-maximizing the government must compare the outcomes of all possible property assignments. Rizzo (1985, p. 873) writes, "A utilitarian or balancing framework would require us to trace the full effects of each (tentative) judicial decision, and then evaluate it

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[6]It is unclear what willingness to pay means when property rights are not held constant. How can people be willing to pay when the money is not theirs to give up?

[7]These points pose problems to other consequentialist theories as well.

[8]This would put the government in the position of the communist regimes that tried to do just that.

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against the particular utilitarian standard adopted." To accurately choose which vector of policies is wealth-maximizing, the government would need to know how every person would act under these new policies—something which would require omniscience on the part of government agents.

Conclusion

While Caplan (1999, p. 836) recognizes that "applying the standard neoclassical Kaldor-Hicks criterion in practice is often difficult," he underestimates the problem. Applying the Kaldor-Hicks criterion is not just difficult in practice; it is impossible. It is unreasonable to assume that judges can figure out what policies are wealth-maximizing. As Rizzo (1980a, p. 291) concluded, "The law cannot and should not aim toward the impossible. Consequently, both the normative and positive justifications for the efficiency approach to tort law must be rejected." If it is impossible to determine what set of laws is most efficient, then surely efficiency should not be the aim of law. In the words of Chipman and Moore:

> After 35 years of technical discussions, we are forced to come back to Robbins' 1932 position. We cannot make policy recommendations except on the basis of value judgments, and these value judgments should be made explicit. . . . When all is said and done, the New Welfare Economics has succeeded in replacing the utilitarian smoke-screen by a still thicker and more terrifying smoke-screen of its own. (1978, p. 581)

Kaldor-Hicks efficiency is an unusable standard. To implement Kaldor-Hicks-efficient policies, government would require wisdom beyond its grasp. Prices only provide evidence of past willingness to pay and cannot be used for formulating future policies. To truly figure out willingness to pay, the government would need to read minds to determine how much every single person would value every possible state of the world, a feat that can never be attained. For these reasons, Kaldor-Hicks should be rejected as a means of judging policy.

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MANAGEMENT VERSUS OWNERSHIP: THE ROAD-PRIVATIZATION DEBATE

LAURENT CARNIS

Roads are frequently cited as a public good even though economists have shown that privatizing the roads would solve some of the problems endemic to public roads, namely, high accident rates, congestion, and pollution (see Roth 1996, 1967; Block 1983, 1979, 1980). Economists have analyzed the problem of externalities and shown how the effects of free-rider behavior can be contained. The question of natural monopoly has been carefully examined, as well. Block (1979) has formulated something of a model showing how a private order would operate. The aim of this present article is to focus on some specific justifications that have been given for government intervention in the market for roads. It will be argued that "commercialization" is not the same as "privatization" and that a private road system must be built on clearly defined property rights.

What are the benefits that can be expect from privatization of the public roads network? It is difficult to answer without engaging in the methodological error of attempting to predict the results of the competitive market process, the very purpose of which is to discover the most suitable means of achieving ends. Economists make their most solid arguments when enunciating general considerations, based on a priori knowledge and general principles arrived at through means of deductive reasoning (Hoppe 1989, p. 1).

At the same time, history provides some illustrative instances of privately built and operated roads.[1] These examples demonstrate how private roads

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[1]Many works have demonstrated the historical viability of private roads including Klein (1996, 1990), Majewski, Baer and Klein (1993), Fielding and Klein (1993), Woolridge (1970), and Taylor (1953).

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might operate in a market economy. The private roads network might consist of a number of different companies competing against each other for customers. Some roads would serve a purely complementary role. The existence of competition would be ensured, not only by the number of roads and road owners, but also from new firms gaining entry into a market uninhibited by legal barriers. Some new roads would be built in response to the desires of consumers, while others would disappear because they are no longer useful or profitable. This private market would discourage the overproduction of roads in specific areas and the underproduction of roads in others. Consumer demand and economic viability would drive and direct the level and type of investment, while profits and losses would reward some production and discourage others. Entrepreneurs, backed by investors from either privately or publicly held companies, would assume the risks and bear the consequences (Kirzner 1996).

There seems to be some confusion, however, in the relevant literature concerning the crucial distinction between full privatization and mere commercialization. Commercialization offers only partial solutions to the problems associated with public ownership. The pro-commercialization economists understand the importance of the market price and market mechanisms but misleadingly recommend it in the absence of property rights (Roth 1996, 1967; Friedman 1989; Vickrey 1963; Brownlee and Heller 1956). For instance, Roth (1996) proposes that the following principle should govern commercial pricing:

> in a market economy, ways have to be found to enable all road costs to be paid for by those who use, or benefit from, roads, and that the amounts payable need not be determined by governments except when road users face monopolistic road suppliers. (p. 104)

The question remains how the price, in these cases, is to be determined, and by whom (Lipsman and Sandler 1996).

The problems associated with government determination of prices (Rothbard 1970) do not disappear when dealing with transportation issues. The general laws concerning price-fixing still apply. When the fixed price is too low, users receive a subsidy that corresponds to the difference between the set price and the market price; resources are underevaluated, and overutilization is the result. When the fixed price is too high, some drivers are obliged to spend more than would be necessary in an open market and, consequently, they decrease their level of consumption. It is certainly true that "it is essential that means of transport be properly priced so as to avoid overallocation or underallocation of resources to transport services as a whole, to particular forms of transport, or to particular segments of any given form" (Brownlee and Heller 1956, p. 249).

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An arbitrary, administrative price provides only a partial answer to the problem of congestion.[2] Instead of reflecting the value judgments of market actors, the price expresses only the preferences of a single individual: the planner. Because public intervention prevents the emergence of market structures that reflect consumer value judgments, the correct market price cannot be established (Mises 1998). An arbitrarily determined price implies huge inefficiencies for producers and consumers and distortions in the allocation of resources.

The lack of attention given to problems of traffic congestion by economists suggests that the issue of "circulation" is generally regarded as an engineering management problem. By using the laws of physics, the engineer tries to regulate the flow of a river to produce electricity. To this end the engineer uses dams, weirs, and other tools. Similarly, in the field of traffic movement, the main problem is managing the flow of drivers. Drivers are considered as atoms, capable of being manipulated and without wills of their own. The variety of ways used to improve road safety is viewed from the same perspective. The wills of individuals disappear behind the experiences of engineers and the decisions of bureaucrats (Cahier des Autoroutes Françaises 1994; O.C.D.E. 1996; Wiel 1966, 1997).

We can find this type of analysis in Roth's work:

> The proper price to charge is the amount equaling the cost of congestion *under the conditions prevailing after the imposition of the new price*. . . . It follows from this that it is not possible to determine the optimal road prices without a knowledge of the reaction of traffic to price variation. (Roth 1966, p. 41, emphasis in original)

The engineering perspective also appears in the work of Vickrey:

> With street use controlled by pricing, however, it is impossible to insure that the level of congestion is kept down to the point at which buses will provide a satisfactory level of service, and rail rapid transit systems will be required only where a volume of traffic arises that will warrant their high cost on the basis of superior service and operating economies. (Vickrey 1963, p. 461)

Roth points out, correctly, that the price must not be fixed arbitrarily in order to determine a prescribed level of circulation, or in order to raise tax revenue (Roth 1967, p.41). But at the same time, Roth seems to share the perspective of an engineer, which is far from an entrepreneurial perspective. Roth denounces one pitfall but falls into another, writing that

[2]Obviously, we could charge a very high price so that only the drivers who could afford to pay would be very few. In that way, we could reduce the level of circulation to a desired level, creating at the same time a huge amount of costs.

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> [t]ime losses are probably the largest component of congestion costs, but congestion also results in increased fuel consumption and vehicle wear-and-tear, and in addition pollution. As a first approximation, all these costs may be assumed to be proportional to time losses, and they can be allowed for in the calculation shown above by appropriate increases in the value of time. It is however evident that the magnitude of congestion costs is critically dependent on the money values assigned to time savings. (Roth 1996, p. 71)

This formulation presupposes the ability to add subjective utility of different people and otherwise make interpersonal comparisons (Lipsman and Sandler 1996). However, it is impossible to make interpersonal comparisons of subjective utility (Rothbard 1956). Moreover, nothing can be asserted concerning the goals that government should follow. Government can seek a particular objective that may or may not have anything in common with the objective of individuals. For instance, decreasing the speed limit can reduce the consumption of oil (a governmental objective) but increase the time required by consumers.

Roth is aware of such limits and thus falls into a kind of skepticism:

> [T]he calculation of congestion charges on the basis of delay costs requires assumptions to be made about the value of time, which is itself a highly contentious issue. . . . As to the preferred method of arriving at the optimal congestion charge, readers can take their choice. Those of us who support market economies are likely to prefer valuation methods based on the interactions of buyers and sellers in competitive markets to prices determined by governments on the basis of academic studies. (Roth 1996, p. 75)

Roth focuses his analysis on different methods of road pricing. He asserts (p. 103) that the charged price must cover the various types of costs he details and is thus inexorably led to repudiate his former attack on fixed prices:

> How do commercial operators decide how much to charge for the goods or services they sell? Much has been written on this subject but the answer generally boils down to two principles: (1) To charge *no less than* the amounts required to meet the "direct" (or out-of-pocket) costs of providing the item in question, and (2) To charge *no more than* prices that maximize profits. (p. 61, emphasis in original)

> The road user should pay a sum equal to the costs he imposes upon others. (p. 106)

On the contrary, the level of prices is determined by the interactions between customers and suppliers. The prices cover some expenses and some costs of production. The difference between costs and revenues rewards the competitive producers with a profit, while applying sanctions to the less competitive ones with losses (Kirzner 1996).

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Roth's perspective leads to the belief that the government can regulate flow, not only by introducing price mechanisms on specific positioning of roads, but also by influencing the rules of the market:

> Even under the commercial management of roads there would be important roles for governments. They should obviously have the last word on where new roads should be permitted (without, however, discriminating against privately provided roads) and on law enforcement. (Ibid., p. 38)

As a proponent of this type of commercialization and not privatization, Roth never deals with the issue of property rights, which would necessarily be violated in this scenario.

Finally, Roth sees the problem in terms of neoclassical economics. His thoughts about the case of monopoly and increasing returns are symptomatic. According to Roth's analysis, this kind of situation legitimizes the imposition of governmental hindrances:

> If the owners enjoyed monopoly power, they could, if they were allowed to, charge more than the costs of providing the required facilities and collect more than the amounts required to keep them in the roads business. In market economies, the prices that may be charged by monopolists are generally regulated, to protect consumers. (p. 75)
>
> It may therefore be concluded that the possibility of natural monopolies occurring in the supply of roads is not an objection to their commercialization, but rather a reason for encouraging commercialization and free entry as the best ways to protect the interests of roads users. (p. 166)

The idea of a natural monopoly is an imaginary construct to justify the establishment of legal monopolies in order to serve particular political interest (Lepage 1989). This theory has received a definitive refutation (Armentano 1999; Rothbard 1993). In the same vein, Roth defends an administrative choice concerning the management of roads networks. The government must manage the market process when one moves from the nirvana situation of a pure and perfect competition model. Governmental hindrances are also justified by the definition of norms with the respect to property rights (Roth 1996, p. 38).

Clearly, then, commercialization of the Rothian variety is not the same as privatization. Neither it is a priori true that commercialization represents an overall improvement over traditional public roads. The introduction of private methods into an essentially socialist structure leads to serious administrative and bureaucratic problems (Mises 1983). For instance, indirect methods of charging are indistinguishable from taxation in the form of surcharges on fuel and an axle-weight-distance tax. Roth's inability to propose a solution constitutes a problem in his major work dealing with commercialization.

> Two approaches have been described in this section for calculating the appropriate charges for the use of congested roads. . . . Would calculations

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> based on these different approaches lead to the same result? If not, which is to be preferred? It is beyond the scope of this book, and the abilities of its author, to plumb the depths of this problem. (Roth 1996, p. 75)

The problem of where and how to invest in future roads is equally serious. Under commercialization, how can it be determined where the infrastructure should be built? Must it be built near other roads or further away? In a system where the prices are controlled and the competition hampered by government interventions, there is no true indicator, no real price. The mechanism of price loses its usefulness, and it becomes impossible to adopt the correct decision. Because there are only administrative prices, economic calculation becomes simply impossible. The impossibility of exchange makes impracticable and unrealizable the proper valuation of resources and hence their correct allocation (Mises 1988, p. 12).

The decision to invest or not to invest is taken by bureaucrats and politicians arbitrarily. Brownlee and Heller insist on the necessity of having criteria because, without them, investments are impossible (p. 237). However, the existence of criteria does not guarantee a correct allocation. A correct allocation becomes possible with the use of unhampered prices, as Mises showed clearly. To decree a criterion that can be called an "administrative price" is no help. It misinterpretations what prices mean and constitutes an arbitrary decision, as the choice of the criterion is itself discretionary (Hoppe 1989, chap. 3).

In economic decisions, the choice of the criterion can imply perverse effects, creating larger distortions in other sectors of the economy. Activities tied to transport, the use of land, the development of cities, and property values are bound up with the initial allocation or misallocation.

> Pricing of street use can in the long run have significant effects on the whole pattern development of urban communities and on property values. (Vickrey 1963, pp. 461-62)

> In practice there are many alternative ways of financing; but no device can function quite as effectively and smoothly as a properly designed price structure in controlling use and providing a guide to the efficient deployment of capital. (p. 455)

These authors understand the role of price (in the abstract) and its effects, but they do not deal with what type of price is necessary to achieve the desired results. These authors often conflate administrative prices with market prices, even though administrative prices are a sure sign of government intervention and do not reflect the choices of users. These authors begin their reasoning in terms of equilibrium within a given network where no change occurs (Roth 1996, pp. 139-40). However, in a market economy, the network of roads must be thought as evolving, with new roads becoming necessary and others becoming useless over time. The neoclassical presentation expresses a confusion

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between the role of a market as a process and its representation as equilibrium (Kirzner 1996). "The object of the 'right' price is to bring about a best level of road usage, i.e., the level at which the costs imposed by vehicles are first equal to the price they have to pay" (Roth 1966, p. 41). But what does the normal return mean? Roth (p. 138) introduces concepts without providing details about their significance. The capital and investment implications of road markets are not analyzed at all in his work.

The complexity and the evolution of the roads network creates new difficulties for governmental agencies and their ability to solve a wide variety of problems (accidents, pollution, traffic, etc.). The government, for example, must solve the problem of information. How can it gather all the knowledge necessary to properly allocate resources? What type of information do the engineers need and to what extent? By presenting the commercialization of roads as the solution to the management problems of the roads network, Roth presupposes that all of the relevant information is given, and he presupposes an ability to analyze it in order to fix the "correct price" for regulating traffic flows.[3] But he never reveals where this information comes from. Consequently, Roth's analysis of the different methods of charging for using roads is unviable (Roth 1967, pp. 45-61; 1996, pp. 59-135). His model makes no room for entrepreneurs who must make judgments based on their expectations about future levels of prices, revenues, and costs, as happens in real-world markets (Mises 1988, p. 12).

In Roth's analysis, the costs are always costs of production, which in turn determine the level of prices. The absence of consumers and entrepreneurs is symptomatic of Roth's analysis, and, indeed, their presence is unnecessary when employing an additive conception of costs. Yet, for reasons of economic calculation and information, an arbitrary administrative price cannot solve the problem. It can only focus on a part of the dilemma.[4] The Rothian system is also a static one. The actual market must be thought of in terms of dynamics and perpetual movement. Some entrepreneurs invest and discover new opportunities of profit and enter the market to supply what the consumers desire (Block 1980; Kirzner 1996; Mises 1998). If they are not competitive, they are displaced by others who supply a better product.

To build a roads system, an administrative price mechanism—commercialization—may yield some solutions to the problems of public roads (congestion, overutilization, etc.), but it gives rise to other problems that cannot be

[3]This level of traffic must be a tradeoff between a level of pollution, risk of accidents, and loss time of driving.

[4]Obviously, we could assign a very high price so that few drivers would be willing to drive. In that way, we could reduce the level of circulation, creating, however, huge costs in other ways.

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solved by central planners. To solve fully the problems associated with public roads, we must envision a system with no arbitrary hindrances and with clearly defined and exchangeable property rights in the roads network and land. If the roads network is to be subject to market discipline, it must be desocialized, not merely commercialized.

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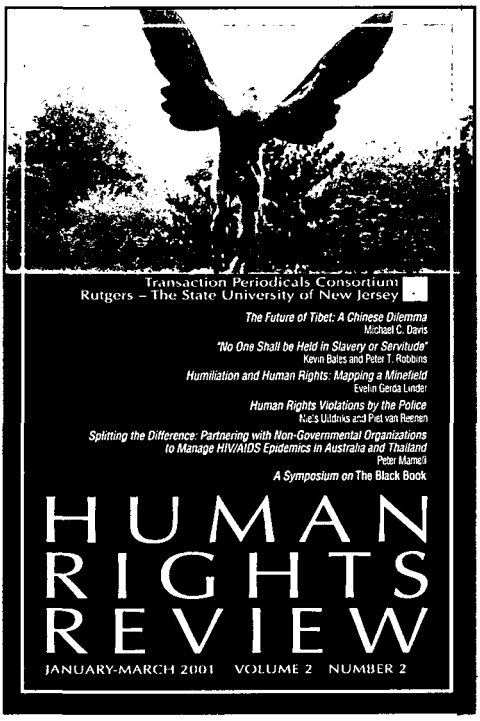
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COMMENT ON CANICE PRENDERGAST'S "A THEORY OF 'YES MEN'"

WALTER BLOCK

Prendergast (1993) has discovered a new "market failure": In the market, "subordinates have an incentive to conform to the opinions of their superiors" (p. 757).

She bases this on three assumptions. First, "the profitability of most organizations depends upon how cheaply they can collect information." Therefore, for any given level of quality, the cheaper the better. Second, "the worker [must] be provided with incentives to collect information" (p. 757) so as to overcome his reluctance to engage in such distasteful activity. Third, there is no objective information against which employee efforts can be measured.

If these three conditions hold, they give rise, in Prendergast's view, to the "yes-man" phenomenon. Here, the worker, instead of ascertaining the facts of the case–that is, the profitability of a new project, the desirability of new machinery or new hires, or the evaluation of a subordinate's productivity–tells the manager what he thinks the manager wants to hear.

This thesis is a more-than-passingly curious one, as the uninitiated would have supposed that any "yes-man" tendency would be summarily dealt with by the market through the imposition of bankruptcy, stock sell-offs, or "hostile" takeovers.[1] After all, profits are earned in accordance with consumer satisfaction, and this can hardly be accomplished if employees, instead of ferreting out accurate information so as to produce high-quality goods and services at low prices, are busily engaged in "yessing" their managers.[2] Management

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[1]Where is Michael Milken? Our need for him has never been more desperate. See Manne (1966a,b); see also Block and McGee (1989).

[2]This applies, for example, to Bill Gates, president of Microsoft, one of the most profitable corporations in U.S. history. Karen Fries, a top executive, describes personal meetings with Gates: "'It's like going to a haunted house. Scary, but fun. Bill loves people who

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cannot succeed unless it "makes the customer an offer he cannot refuse." If instead of helping the managers do this, workers tell them what they think they want to hear, this will, at the very least function as a competitive disadvantage. It therefore behooves us to delve into the reasoning of Prendergast, to determine just how she is led to such a counterintuitive supposition.

There is no problem with the first of her three assumptions. The importance of collecting accurate information in an expeditious manner has a strong pedigree within the profession.[3] Nor can there be any serious objection to the second assumption. Work has disutility, and the principle-agent problem must somehow be overcome. But the third presents difficulties. Why should there be no objective information against which workers' efforts can be evaluated? Or, to express this in terms more amenable to economic analysis, where is the evidence that the costs of unearthing such information would be more costly than the losses of "yes-manning"?

Prendergast offers us three reasons why, in effect, it would be too costly for the manager to "wait and see if the project is profitable before deciding how well to reward the worker." More explicitly, she specifies what the problem is with trying "to obtain more objective information on the true parameter before rewarding the worker" (p. 757).

First, it may be "difficult" to "obtain an objective measure of the worker's input" (ibid.). But this is just like estimating the marginal-revenue product of actual and potential employees. Of course it is difficult to do: there are joint products; productivity depends upon how the worker "fits in" with others; it is impossible to keep one's eye on a given person all day long; etc. But this challenge occurs in every nook and cranny of the economy. Why this obstacle should be elevated to the status of a market failure is not at all clear. Nor is the solution theoretically very formidable: those entrepreneurs who can carry out such tasks prosper; those who cannot, do not.

Second, some amount of time may pass between the worker's task being completed and the reward (or penalty) being imposed. This calls for "commitment" and "institutional memory" (ibid.). Again, this does not seem to be an insurmountable barrier. There can be no denying that we live in an imperfect world; it simply does not pay to memorize trivial details. But we are vouchsafed no reason to believe that most firms do not set their rewards and penalties at least roughly in accord with the actual accomplishments of their employees—that they do not "remember" who was naughty and who was nice when it comes time, much later, to hand out bonuses, raises, and promotions.

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stand up to him. He hates yes people.' At Microsoft, she adds, everything depends on how well you 'defend your point of view'" (*Newsweek*, 11 July 1994, p. 41).

[3]See Hayek (1948), Stigler (1961), Kirzner (1973), Klein (1996, 1999), Foss (1994), Lewin (1999).

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When firms do not set wages and perks at least roughly comparable to productivity, labor mobility tends to push the market toward equilibrium in this regard. The worker who feels unduly belittled can leave; the firm that feels cheated by the employee can fire him. There is always a passage of time between work and reward, moreover. What makes this case special?

Third, Prendergast posits some sort of radical unpredictability[4]: at the time the worker made his decision, he had no way of knowing whether he was right or wrong, because there was "noise." Project profitability, for example, depends upon so many other people and phenomena (e.g., the weather, technological change, etc.) "in a way that cannot be predicted when the initial inference is made" (pp. 757-58). But Knightian uncertainty is a concomitant of all entrepreneurial activity. We must again ask, why is this case so special? No one can ever predict for sure; if they could, the most extreme versions of rational expectations would be true, and there would be no need for a market at all.

But even these extreme assumptions will not yield the conclusion desired by this author. If there are one hundred workers, each making predictions with no ostensible justification at the time they were made, still, some will do better than others. These are the ones who will tend to be better rewarded by the marketplace.

Further, there is always the expedient of manufactured prediction. A firm interested in the ability of its labor force can very cheaply administer a test[5] to its workers, where the correct answers are known in advance only to the manager.[6] Under the conditions posited by Prendergast, the management would have every incentive to hide[7] its true opinions. In this way, it could nip "yessing" in the bud.

In any case, it is imperative for the Prendergast thesis that there be "no obvious metric by which to measure whether a worker's report is correct" (p. 758).

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[4]See Lachmann (1977, 1986) for an elaboration of the thesis that the future is inherently unknowable.

[5]Certainly, corporations go to great lengths to attempt to measure performance, and reward it accordingly, lest they suffer an excessively high quit rate precisely for those employees they wish to keep on the basis of these exams. If "yesmanning" was so pervasive, firms would hardly engage in such activities. For more on this see Brickley (2001). I owe this point to an anonymous referee.

[6]It may be difficult for the orchestra conductor to know which instrumentalists are carrying their full weight and which are shirking. After all, during the cacophony of a symphony, it may be difficult to tell one from the other. But if there are problems here, there are remedies, too. First, conductors are hired particularly on the basis of being able to separate the sheep from the goats even when there is a lot of "noise" occurring. Second, he can always ask each member of the orchestra to play by himself, during practice, in an attempt to "objectively measure" the various contributions of the musicians. The various artificial tests of predictive power would be the business analog to the musical case.

[7]Or perhaps even better, falsify them (so as to trap the unwary).

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She needs this conclusion in order to explain the rise of "yesmanship." For if there are objective criteria, telling the manager something false in order to butter him up is unlikely in the extreme to be a successful strategy.[8]

She states:

> In the absence of reliable objective measures of performance, firms generally resort to subjective evaluation procedures, where managers compare their own findings to those of their subordinates. When the point of reference for adequate performance is the manager's opinion, an endogenous desire arises for the worker to conform to the opinion of the manager. This arises because the only way to induce the worker to exert effort is by comparing the findings to the manager with those of the worker. (p. 758)

There are problems here. Why is it that firms only "generally" resort to subjective evaluation procedures? Under the assumptions imposed by Prendergast, there is no other alternative but to act in this way. Certainly, she has not left room for any objective criteria. The fact that she falls short of writing consistently with her own theory may perhaps indicate that even she does not fully take it seriously.

Notice, too, the almost imperceptible change from "opinion" of the manager in the second sentence quoted above to her "findings" in the third. Only one of these can be correct. If we are truly in a kaleidic (Lachmannite) world, where the future is absolutely unknowable, then there can only be "opinions" about what is to come, not "findings." To the extent that there are indeed "findings," we have not moved entirely from the real world to one of Prendergast's construction.

Most importantly, there is nothing wrong with yesmanship under Prendergast's unrealistic assumptions! Let us assume with her that there are no objective criteria for correctness in prediction. But let us also take it for granted that the manager is somehow best able to discern things in this fog that are reality[9]—after all, he wouldn't be manager if he didn't have some sort of comparative advantage over his workers. Under these artificial conditions, it is a sign of productivity that the workers be able to anticipate the manager. Under these extreme assumptions, yessing, which is ordinarily counterproductive, becomes, paradoxically, productive.[10]

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[8]With the exception of cases where the manager is using compliance of his underlings as a consumption good. But in this case, it is the function of the CEO to root out such behavior. And if this somehow fails to occur, there is still the fail-safe market mechanism of lower stock prices, "hostile" takeovers, and bankruptcy.

[9]"In the land of the blind, the one-eyed man is King."

[10]In the case of cooperative music playing, "yessing"—doing exactly what the conductor wants even if this means repressing one's own individual interpretation—is precisely the way to maximize production. The conductor is to the orchestra as the pianist is to the

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Prendergast wants to have her cake and eat it too. She gives us such unrealistic assumptions that "yessing" actually becomes productive. Under these assumptions there is nothing left for the worker to do except agree with his boss. It is impossible for "the worker to act honestly" (p. 769) if we interpret "honest action" in its usual meaning so as to obviate the possibility of agreeing for the sake of agreeing. For honesty means telling the truth without fear or favor, whether or not the boss agrees with the truth. But in the artificial world constructed by Prendergast, there is no truth separate and apart from the manager's opinion. On the contrary, it is his views which define truth. That is why it is impossible to "be honest" without "yessing."

There are three main responses to the Prendergast thesis. One, we do live in a rational world where it is possible to predict (even though there are no guarantees of success). Here, the decisions of workers can be objectively evaluated, and rewarded or punished accordingly, even though this cannot be done costlessly. In this model, there is simply no room for the yes man. The market will continually root out this behavior, as it is incompatible with ascertaining the facts on the basis of which consumers can be satisfied.

Two, Prendergast is right: the world (or at least a significant part of it) is essentially causeless; nothing can be predicted; "there is no obvious (or even nonobvious) metric by which to measure whether a worker's report is correct" (p. 758). Here, the only thing to go by is the opinion (not "finding") of the manager. But in this case, it is perfectly "reasonable" to engage in yes-man behavior. This is because of an implicit premise that the boss knows more than the worker and that, therefore, the way to increase productivity is to somehow train the latter into being able to mimic the former. That is to say, "yessing" now, paradoxically, becomes productive.[11]

It is rather disingenuous of Prendergast, therefore, to claim that "the incentive to conform implies inefficiencies . . . and is likely to lead to more centralized decision making than in the absence of the desire to conform" (p. 757). The incentive to conform implies inefficiencies, but only in the real world—the one in which market forces continually grind down all such tendencies. In her artificial world, conformity with the boss does not at all imply inefficiency; rather, the opposite. Nor is it true in the model she has constructed that conformity "will lead" to more centralized decision-making (e.g., the boss will do things his way, and ignore the advice of the workers). Instead, this is logically implied by her model. It follows from the fact that there are no independent

keys; and, just as the last thing the pianist wants is keys with "individuality" that "think for themselves," so too does the conductor strive to promote "yesmanship" amongst the orchestra members. (This, of course, does not apply directly to soloists, an entirely different and more complex matter.)

[11]It becomes productive solely due to the force of Prendergast's unrealistic assumptions.

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criteria upon which an opinion different from the managers could possibly be justified.

From this statement, also, it would appear that the desire to conform is exogenous. This is indeed true in the real world, where the market is busily engaged in stamping it out, exactly as it treats all other forms of antiprofitable behavior (shirking, cheating, stealing, etc.) But in the Prendergast world, "yessing" is not exogenous. On the contrary, it again follows from the "fact" that there are no independent standards with which to judge the worker's decision-making. All that can be done in this case is to rely on the manager's opinion—and on the ability of the worker to ferret it out of him and replicate it.

There is a third reply to Prendergast as well.[12] The not-too-implicit implication of her theory is that government intervention is justified in order to eradicate, or at least reduce, the inefficiency of the market failure of yesmanship. After all, given the market-failure literature, to characterize something as a member of that category is to call for the state to come in and fix it.[13] But it is within the bowels of government where the real yes-men problem lies. Here, there is no automatic feedback mechanism of the market to rely upon, to quell any incipient tendencies in the direction of yes-manning. At best they are extraordinarily weak; at worst they are nonexistent.[14] Thus, far from Prendergast being able to make her point that the yes man phenomenon is a market failure, the truth of the matter is that it is a government failure. If market firms rely upon or are involved in yesmanship, they lose money and tend to go bankrupt; if and to the extent that governments become embroiled in this economic virus, the forces for their dissolution are either very much attenuated, or totally absent.

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[12]This was suggested to me by an anonymous referee. I thank him for it.

[13]See Cowen (1988).

[14]True, the Public Choice School posits that the political process constitutes a feedback mechanism that would be able to ameliorate yes-man tendencies; see on this Buchanan and Tullock (1971), Dauterive and Barnett (1984), Grier and McGarrity (1998), Holcombe (1986), McGarrity and Lloyd (1995), O'Brien and Logan (1989), Sutter and McGarrity (2000), DiLorenzo (1988). The ballot-box vote, however, is a very imperfect substitute for the dollar vote. The former occurs, typically, once every four years and is in effect limited to a package deal offered by two not dissimilar political parties; the latter occurs every day and has no such limits. Further ruining the analogy, the political process is based on compulsion, the market on voluntary interaction.

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PROBABILITY, COMMON SENSE, AND REALISM: A REPLY TO HÜLSMANN AND BLOCK

BRYAN CAPLAN

In my critique of Austrian economics[1] (Caplan 1999), I carved out a virtually unique position: Despite the Austrians' professed devotion to "realism" against neoclassical pragmatism, the latter approach is in fact far more realistic than the former. My critics (Hülsmann 1999, Block 1999) remain unconvinced. The editor's invitation to respond provides me with an ideal opportunity not only to defend myself, but to spell out the philosophical side of my original thesis in greater depth.

PROBABILITY

Block calls the treatment of probability "the weakest section" (1999, p. 30) of my original paper. By itself, this spurs me to try to strengthen it. But more fundamentally, reading my critics convinces me that the issue of probability permeates every aspect of this discussion. Block, for example, observes that the envious "have no way to *prove* they lose out at all," that "scientifically we can know *nothing* about states of mind not demonstrated in action," and, regarding latent preferences, asks, "How can we, as economists, even know they exist?" (1999, pp. 32, 34, 23; emphasis mine). Hülsmann, similarly, remarks that "the very existence of the innovator prevents *any attempt* to establish regularities of what human beings choose," and that "there is *no way* to judge whether people really want a good, and how much they want of it, other than by looking at their actions" (1999, pp. 13, 16; emphasis mine). On

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[1]Throughout this paper, the term "Misesian" is used to specifically refer to the strand of Austrian economics primarily influenced by Mises and Rothbard, and "Austrian" to jointly refer to Misesian and non-Misesian Austrians alike. Both Hülsmann and Block obviously fall on the Misesian side, so most of the comments will be directed accordingly.

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topic after topic, Block and Hülsmann make extreme claims about how little people are able to know.

The link to probability theory is fairly simple. Once we rule out all talk about "degrees of probability" for practical questions (as opposed to idealized games of chance), the pettiest doubt can be used to trump the "scientific" merits of an array of mundane observations. Take "Socialists envy the rich," for example (Mises 1972). After refusing to assign a probability to this assertion, it is easy to slide from "There's no way to be certain," to "Not proven," to "No scientific basis," to "No way of knowing." Austrian critiques of neoclassicism often come down to this generic complaint.

Calling an objection "generic," needless to say, hardly shows that it is wrong. In Caplan (1999), my central counter to the full-blown Misesian rejection of quantifiable probability was a reductio ad absurdum. If we cannot quantify the probability of an "individual, unique, and nonrepeatable" (Mises 1966, p. 111) event, then we can never quantify probability at all, because strictly speaking, *all* events are "individual, unique, and nonrepeatable." Naturally, though, this reductio is only persuasive if, ex ante, you acknowledge the absurdity of rejecting all real-world applications of probability theory.[2]

This indirect argument can be complemented, however, with a direct defense: The basic principles of probability are simply self-evident. It is self-evident that one holds beliefs with *some degree* of certainty.[3] It is self-evident that the degree of belief must vary from *impossible* to *certain*. It is self-evident that intermediate degrees of belief can be compared to each other (Bush was more likely to win than Gore) and to any point on the probability spectrum (Bush was more than 50 percent likely to win). These self-evident truths have

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[2]Hülsmann is apparently willing to deny that there is anything absurd here: "*even if* [Caplan] were correct, it would be a confirmation rather than a problem for the Austrian approach" (1999, p. 12). Block, responding with greater restraint, states that "at worst it only attacks the minor Austrian point" (1999, p. 29). But then Block goes on to affirm that probability theory can still sometimes be used for real-world problems: "It all depends upon just how 'dishonest' are the roulette wheels. If only slightly so, then class probability would still apply" (1999, p. 29). But if people can keep using class probabilities in spite of "slight" unique aspects, what prevents them from using them (with less and less success) as the unique aspects expand indefinitely? Moreover, calling a deviation "slight" is itself a quantitative judgment about a unique event!

[3]Part of the Misesian complaint about probability theory seems to stem from a conflation between objective and quantifiable probability. Hoppe, for instance, objects to the view that we possess "perfect knowledge of the probability distributions of all future classes of actions" (1997, p. 56). On the subjective-but-quantifiable view of probability, however, "perfect knowledge of probability distributions" is equivalent to "perfect knowledge, upon reflection, of one's degree of confidence in one's own beliefs," not "perfect knowledge of carefully prepared actuarial tables for all possible events."

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a wealth of corollaries, like: Lack of full certainty is not equivalent to no knowledge at all.[4]

One probability judgment I make is that flat-footed appeals to self-evidence are unlikely to win over my critics.[5] But perhaps I can slightly raise this likelihood by couching these claims in strict praxeological terms. Mises writes:

> Acting requires and presupposes the category of causality. Only a man who sees the world in the light of causality is fitted to act. In this sense we may say that causality is a category of action. The category *means and ends* presupposes the category *cause and effect.* In a world without causality and regularity of phenomena there would be no field for human reasoning and human action. Such a world would be a chaos in which man would be at a loss to find any orientation and guidance. Man is not even capable of imagining the conditions of such a chaotic universe. (1966, p. 22)

But is causality really implicit in the axiom of action? Is it really the case that "[I]n order to act, man must know the causal relationship between events, processes, or states of affairs" (Mises 1966, p. 23)? Not quite. Mises's claim is too strong; what action really supposes is not *causality* but *probability.*[6] In order to act, one merely needs to believe that one's action produces a *better distribution* of outcomes than any alternative action. There is no need to believe that one's action will succeed, or is even very likely to succeed. For example, my action of submitting a paper to the *American Economic Review* does not depend on the strict *causal* belief that "Submitting papers to the *AER*

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[4]Hoppe (1997) forcefully asserts this truism several times, but juxtaposes it against various extreme skeptical claims, such as "any progress in [man's ability to predict his own future actions] must be considered systematically impossible" (p. 60).

[5]Even so, as David Gordon astutely observed in another context:

> I have claimed that certain moral judgments are obviously true: but they are certainly not obvious to [L.A. Rollins, a critic of Rothbard's natural rights theory]. Fortunately, it is not an argument against a moral theory's truth that L.A. Rollins fails to believe it. That is a fact of purely biographical significance. (1988, p. 233)

[6]Mises interestingly discusses "statistical laws" in the same section, leaving it somewhat unclear as to whether he acknowledges such a thing as "statistical causality." But Hoppe explicitly endorses a deterministic interpretation of causality:

> [O]ne establishes that phenomena have such causes by following a particular type of investigative procedure, by refusing on principle to allow *any exceptions*, i.e., instances of inconstancy, and by being prepared to deal with them by producing a new causal hypothesis each time any such an apparent inconstancy occurs. (1989, pp. 113-14; emphasis mine)

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always causes acceptances." I only need to believe that "Submitting papers to the *AER is*–all things considered–better for me than sending them anyplace else."

In other words, Mises is correct to point out that beliefs about the efficacy of action are implicit in action. But he at best misspeaks when he characterizes this necessary feature of action as knowledge of "causality." Instead, the necessary belief component of action is weaker; we don't need to know–or even believe we know–any exceptionless causal laws. We merely require beliefs about conditional probabilities.

As with other fundamentals of action, doubts about probability are self-refuting. One cannot even argue against probability theory without implicitly holding some belief about the probability that doing so will change listeners' minds. Few Misesians would be naive enough to imagine that attacking probability theory *has to* reduce its number of adherents. Rather, they attack probability theory because, given their beliefs about the probability of changing people's minds, they see it as the most valuable way to use their time. Any attack on probability presupposes it.

But how can these claims about probability be reconciled with realism? Hülsmann (1999, p. 12) makes the fair point that university professors spend far more time explicitly calculating probabilities than businessmen (though, contrary to Hülsmann, even moderately sophisticated businessmen habitually compute expected present discounted values using elementary probability theory). To this, I respond that a probability assessment is exactly analogous to a willingness-to-pay. People may be unable to *articulate*, for example, that "I would be willing to pay $200 per month in additional rent to live in a safer neighborhood." They might even nonsensically assert that "You can't put a price on safety." But in acting, they implicitly make such trade-offs. Similarly, people may be unable to articulate that "I believe the probability of being murdered in my neighborhood is .001 percent per year," and they might evasively respond, "I just don't know." But in acting, they implicitly set probabilities. If they thought the probability of being murdered was 90 percent per year, they would move; conversely, if they thought the probability was 0 percent, they would stop wasting time on ordinary precautions. In short, just as demand theory does not commit us to the view that the typical person explicitly ponders, "How much Gouda cheese would I buy if the price were a penny per pound?" probability theory does not commit us to the view that the typical person explicitly ponders, "What is the probability that I have an evil twin?"

Incidentally, Hoppe (1997) raises two other noteworthy objections to the realism of probability theory. The first is that it implies that "it would be possible to give an exhaustive classification of all possible actions. . . . For without a complete enumeration of all possible types of actions there can be no knowledge of their relative frequencies" (p. 56). There is a simple reply: One

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can always complete an enumeration by adding a catch-all "other" category. In assigning probabilities to the party affiliation of the next president of the United States, for example, one need merely break the possibilities down into Democrat, Republican, and "other." Hoppe's second objection points to the reality of systematic error (1997, p. 57). But this conflates a fundamental tenet of neoclassicism–subjective-but-quantifiable probabilities–with a popular subsidiary neoclassical hypothesis–rational expectations (1997, p. 56). Rational expectations is a hypothesis *linking* subjective-but-quantifiable probabilities to objective frequencies. Empirical evidence of systematically mistaken beliefs (Caplan 2001a) counts only against rational expectations, not probability.

If they were to change their positions on probability at all, I bet that Hülsmann and Block would embrace my view, not an intermediate position like Kirzner's. Still, it is worth noting the weakness of Kirzner's account from a praxeological standpoint. Kirzner often speaks of opportunities that have been "utterly overlooked" (e.g., 1997, p. 71). When we bear in mind my observation that every action presupposes probabilities, however, this strongly suggests that Kirznerian "utterly overlooked" possibilities are neither more nor less than events with a perceived probability of 0. They are not counter-examples to the usual neoclassical approach. To act while "utterly overlooking" X is praxeologically equivalent to acting on the belief that X is impossible.[7]

Hülsmann offers two main arguments against my wholesale rejection of Knightian uncertainty. One is the role of ideas in human history:

> [T]he invention of new ideas again and again changes the way human beings act under otherwise equal circumstances. . . . The very existence of the innovator prevents any attempt to establish regularities of what people choose. (1999, p. 13)

But there is no reason the invention of new ideas cannot be assimilated into a standard probabilistic framework. We don't expect big changes to happen every day; or, in other words, we assign low probabilities to big innovations. Moreover, the effects of big innovations are often quite predictable. When the automobile suddenly appears, it doesn't take a genius to realize that people will move further away from their places of employment, that gas stations will spring up, and so on. Even when predictions are highly inaccurate, that is different from *totally* inaccurate. Few futurists foresaw the extent of progress in personal computing, but at least they correctly guessed that computers would sharply reduce the fraction of human labor devoted to rote activities.

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[7]As I observe in Caplan (1999), this insight raises serious doubts about the empirical relevance of Kirzner's approach. If you really thought that the probability of X was 0, the apparent occurrence of X would not lead you to change your mind about X. Instead, you would have to conclude that you were mistaken or delusional.

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Indeed, it is trivial to list an array of "regularities of what people choose" that have persisted unchanged in spite of the entire history of innovation: Most people are selfish most of the time and give a small fraction of their income away to strangers. Most people will eat rather than starve if they have access to food. Most people are heterosexual. Males are, on average, more attracted to younger women. Most people would pay a large amount to escape an otherwise uninhabited island. Sociobiology provides us with a great many less jejune generalizations about human choices (Dawkins 1989). The claim that "the emergence of new ideas *necessarily* overthrows *all* previous patterns of behavior" (Block 1999, p. 31; emphasis mine) is a wild exaggeration.[8]

Hülsmann's second argument, which he attributes to Hoppe, is that "any determinist theory of choice (like the probabilistic approach) implies an inescapable contradiction." Why? It

> presupposes that man *cannot* learn because any adoption of new ideas would change the way he acts. . . . However, presupposing that man cannot learn contradicts the necessary assumption of research activity, namely, that research will make a difference. (1999, p. 13)

In spite of my firm belief in free will, though, this contradiction appears readily "escapable." Suppose, for example, that my determinist theory is that every extra hour of study on the SAT will raise my score by exactly three points. This is a determinist theory of learning, and it hardly contradicts the possibility of learning. The determinist can forsee *that* he will learn, and *how* his learning will improve his performance, without already *knowing* the unlearned material. Students do this all the time.[9]

That aside, classifying probabilism as a "determinist theory of choice" is sleight of hand. Probability theory *explicitly* allows for learning; that is what "Bayesian updating" is. Suppose I believe "There is an 80-percent probability that I get tenure next year. If I get tenure, I will stay where I am; otherwise, I will find a new job someplace else." The next year comes along, I reach "new ideas" when I *learn* whether I get tenure, and depending on the outcome, I change the way I act. Even if Hoppe's argument against determinist choice theories were correct, using it to argue against probabilistic choice theories makes about as much sense as using contradictions in the Bible to argue against Buddhism.

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[8]Hoppe (1997, pp. 72, 77), in contrast, broadly acknowledges the existence of "regularities of what people choose," though ultimately this is difficult to square with his strictures against extending the scientific method to the study of human beings (1997, pp. 60-61).

[9]Hülsmann's formulation also reflects a sharp departure from subjectivism. Why couldn't a researcher be motivated by idle curiosity, rather than by an instrumentalist desire to "make a difference"?

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In sum, then, the basics of probability theory are self-evident and, rightly understood, are highly intuitive. Knightian uncertainty is incoherent. More importantly for Misesians, probability theory deserves to take the place of causality as a fundamental implication of the action axiom. Any attempt to deny probability theory inevitably winds up presupposing it.

Common Sense[10]

Another basic philosophical difference between myself and my critics is the relation we see between common sense and science. Block repeatedly draws this sort of distinction: "*In ordinary language*, a person could be readily understood to be indifferent between wearing a green or a blue sweater." "Sure [latent preferences can exist], in the *ordinary commonsensical* notion" (1999, pp. 22, 36; emphasis mine). Hülsmann remarks that "Caplan's errors seem all to be rooted in his failure to grasp that Austrian economics is a theory of action (praxeology) rather than some kind of applied psychology" (1999, p. 4).

Simple question: Is the "common-sense," "ordinary-language" belief *true or not*? If it is true, then it makes no difference whether the belief is "praxeological." Austrians have rightly lambasted neoclassical economists for their narrowness; why would they want to imitate them? Naturally, one might draw attention to the nuances that distinguish praxeology and, say, "applied psychology." But it is hard to avoid the impression that my critics label claims as "nonpraxeological" in order to somehow exclude them from the discussion. This is particularly distressing when praxeology and "applied psychology" can be joined together to yield new insight. Mises, Rothbard, and even the "extreme rationalist" Hoppe (1989, p. 1) all acknowledge a role for empirical assumptions to supplement pure praxeology.[11] Why might not the same hold for any of the propositions of "applied psychology" I purportedly advance?

Block puts forward the most cogent potential answer. As he explains, "other academic disciplines also utilize ordinary words and invest them with technical meaning. . . . [T]he physicist means one thing by 'work,' . . . while the man in the street means something somewhat different" (1999, p. 22). But this raises a disturbing possibility for Misesians. Could many of their long-standing disputes with neoclassical economics be a trivial product of divergent technical

[10]While this section focuses on Misesian departures from common sense, non-Misesian Austrians are at least equally guilty on this count. As Hoppe (1997, p. 52) asserts, "To claim . . . that we are faced with radical uncertainty and that the future is to all of us unknowable is not only self-contradictory but also appears to be a position devoid of common sense."

[11]Rothbard (1962) emphasizes three empirical assumptions: the value of leisure (p. 37, and particularly, p. 437, n. 27), human and resource variety (p. 82), and rate-of-return maximization (pp. 377–78).

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definitions? Particularly if neoclassical usage accords with ordinary language, Austrians would have only themselves to blame for any misunderstanding. Take, for example, the Misesian critique of indifference analysis. Suppose common-sense claims about "indifference" were *really* compatible with Rothbard's views on the subject. Then when he accuses neoclassicals of subscribing to "fallacies" on this point, they could fairly respond, "No, the neoclassical concept of indifference is identical to the common-sense one."

My point here is *not* that neoclassical-Austrian disputes *are* purely semantic; rather, my point is that *if* apparent conflicts between Austrian claims and common sense were purely semantic, as Block suggests, *then* it would be reasonable to suppose that neoclassical-Austrian disputes were purely semantic as well. If we deny the latter, we must deny the former, too. The implication is that Austrian claims and common sense often *really are* at odds. Austrians must in consequence say that they are right and common sense is wrong. In spite of disclaimers to the contrary, I believe that this is the real Austrian position.[12] The most charitable stance they can take vis-à-vis incompatible common-sense claims is that the latter are colorful metaphors, not literal assertions.

It is easy to imagine many Austrians biting this bullet: "So common sense is wrong. One important function of science is to correct popular confusions." Or, as Kant scoffed at Thomas Reid and other "common-sense" critics of Hume:

> They found a more convenient method of being defiant without any insight, viz., the appeal to *common sense*. It is indeed a great gift of heaven to possess right or (as they now call it) plain common sense. But this common sense must be shown in deeds by well-considered and reasonable thoughts and words, not by appealing to it as an oracle when no rational justification of oneself can be advanced. . . . Seen in a clear light, it is but an appeal to the opinion of the multitude, of whose applause the philosopher is ashamed, while the popular charlatan glories and confides in it. (1977, p. 851)

To this, I respond that Kant was wrong and Reid (1940, 1872) was right. The whole point of "proofs" is to move from the known to the unknown, from the more probable to the less probable. Fundamental principles of common sense cannot be "proven" *because they are obvious*; there is nothing more probable to derive them *from*. As Reid brilliantly puts it,

> [W]hen we attempt to prove, by direct argument, what is really self-evident, the reasoning will always be inconclusive; for it will either take for granted the thing to be proved, or something not more evident; and so, instead of giving strength to the conclusion, will rather tempt those to doubt of it who never did before. (1872, p. 637)

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[12]Rothbard amusingly pointed out heterodox movements' tendency to camouflage unpalatable "esoteric" doctrines with crowd-pleasing "exoteric" doctrines (Raimondo 2000, pp. 122-23).

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Philosophers who miss this point wind up, like Hume, in abject skepticism, or, like Kant, constructing enormous systems of non sequiturs in a desperate attempt to avoid skepticism.

The only way to challenge common-sense beliefs is to show that they conflict with even more obvious common-sense beliefs.[13] In the natural sciences, careful observation has occasionally shown that "the obvious" is wrong. But this only succeeds by relying on an even more basic principle of common sense: the validity of the senses. In the social sciences, however, conflicts with common sense are far more suspicious. The fundamental principles of the natural sciences are frequently beyond the experience (and comprehension) of ordinary people; but as Austrians have often reiterated, the fundamental principles of the social sciences are omnipresent. As Rothbard observes,

> Whereas in physics, causal relations can only be assumed hypothetically and later approximately verified by referring to precise observable regularities, in praxeology we *know* the causal force at work. This causal force is human action, *motivated*, purposeful behavior, directed at ends. (1962, p. 277)

The literal existence of indifference, for example, is not a mere hypothesis; we *know* it is real from introspection.

My critics implicitly make a distinction between common sense and praxeological truths. If this distinction is simply disciplinary, Austrians ignore common sense at their peril. If the distinction is semantic, there is every reason to think that many apparent conflicts between neoclassical and Austrian economists are semantic as well, rendering decades of Austrian objections quite hollow. And if, as I think, common sense and praxeology literally disagree about basic questions, common sense has priority.

Specific Replies

Building on these foundations of probability and common sense, I now turn to specifics of the Hülsmann and Block critiques. Due to their high (not to say perfect) level of agreement, I mostly respond topic by topic, rather than author by author.

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[13]Misesians might be tempted to reply that the action axiom has a probability of one and is consequently able to trump common sense. But this claim itself presupposes various common-sense premises about the reliability of one's intellectual faculties. Moreover, even the apparent implications of a perfectly certain axiom must be less than fully certain due to the fallibility of deduction. Suppose that one's deductions about, say, indifference, conflict with common sense. Which is more likely? That an error has slipped into an extended chain of abstract reasoning, or that our every introspective experience of indifference is illusory?

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Indifference

Here I have little to add to my original discussion. Common sense and neoclassical theory say we can be indifferent; Misesians say we cannot. Contra Block (1999, p. 24), the dispute is hardly a semantic one between praxeology and psychology. I can imagine being indifferent in a praxeological sense (I have no strict preference between *a* and *b*) while having diverse psychological states. Rather than having no strong feelings about green versus blue sweaters, for example, I might have intense but conflicting feelings about both, which, coincidentally, leave me indifferent. Block wonders, "Why would anyone bestir himself if there were absolutely no gain in it for him" (1999, p. 24)? But one could just as well inquire, "Why would anyone *fail* to bestir himself if there were absolutely no cost to him of doing so?" Normally, moreover, one is only indifferent between some options; I can be indifferent between green and blue sweaters, but strictly prefer one sweater to none. As Rothbard explains, "Buridan's ass . . . is confronted not with *two* choices, but with *three*, the third being to starve where he is" (1962, p. 267).[14]

Block's attempt to show that my position is self-refuting is unimpressive. "For this George Mason economist is not indifferent between the Austrian (anti-indifference) and the neoclassical (pro-indifference) theories" (1999, p. 24). True enough; however, my thesis is not that we are *always* indifferent, but that we are *sometimes* indifferent. Similarly, Block's deduction that "For Caplan, it might be that one of the trading partners, or, even, neither of them, made any gain in utility," is quite true. But that "might" is crucial. So what if one-in-a-trillion exchanges strictly raises the utility of neither participant?

Cardinality

Hülsmann argues that a necessary condition for the existence of a ratio is identity of units: A ratio could only exist if "preference ranks and prices had the same dimension (that is, if they were the same kind of thing)" (1999, p. 8). In general terms, this is simply wrong. A ratio of two lengths (2 feet: 1 foot) can equal a ratio of two timespans (2 centuries: 1 century). The units on both sides of the equality cancel. But for neoclassical utility functions, even this point is superfluous, for the simple reason that utility functions are *unitless*! We can divide an abstract function $f(x)$ by an abstract function $g(x)$ even though neither has any units "attached." Hülsmann goes on to list two supposedly absurd ratios: "a rabbit divided by a piano concerto . . . a combustion engine divided by a prayer" (1999, p. 9). Amusing as these examples are, there is nothing

[14]Block strangely footnotes this passage after telling us, "If indifference were his exact mental state, surely he would select neither article of clothing" (1999, p. 22)! Rothbard, in contrast, correctly infers against Schumpeter that "*Even on the indifferentists' own grounds,* this third choice will be ranked lower than the other two on the actor's value scale. He will *not* choose starvation" (1962, p. 267; first emphasis added).

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wrong with them. How do they differ from "miles divided by hours" (a ratio of distance to time) or "pies divided by children," (a ratio of pies to children)? Finally, in noting that "prices are themselves ratios," Hülsmann neglects the standard assumption that both prices are expressed in terms of a numeraire.[15]

Continuity

I agree completely with Block that continuity (and differentiability) assumptions have occasionally been misleading, for example in the theory of monopolistic competition. But this justifies only caution, not wholesale rejection. Block interestingly excuses Rothbard's habitual reversion to intersecting supply-and-demand diagrams as follows: "Surely, after taking great pains to show discontinuities, he may be forgiven for lapsing into the traditional analysis, if only as a heuristic device" (1999, p. 29). Block seems to forget that neoclassical economists have spent a staggering amount of brain power (indeed, in my judgment, far more than the marginal benefits warrant; see, e.g., Hildenbrand and Kirman 1988) spelling out the dependence of standard analysis on the assumption of continuity. If Rothbard's "great pains to show discontinuities" grant him a license to draw intersecting supply-and-demand curves, neoclassical economists deserve the same privilege.[16]

Hülsmann arguably goes further than Block by firmly endorsing the conclusion that "market equilibrium . . . cannot be *represented* as the intersection of supply and demand curves" (1999, p. 10). This position is so counter-intuitive that, contrary to Hülsmann, he is virtually the only economist to ever embrace it. *Human Action* is graphless, but Mises repeatedly refers to "intersections" of supply and demand, admitting, for example, that "It is possible to visualize this interaction by drawing two curves, the demand curve and the supply curve, whose intersection shows the price" (1966, p. 333).[17] The near-impossibility of talking about supply and demand without alluding to their intersection reveals how contrary to common sense Hülsmann's position is.

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[15]Alternately, as Murphy (2000) astutely notes, we can include the appropriate units on *both* the left- and right-hand sides of the equation: "That's what *marginal utility* means, after all: the increase in utility . . . resulting from an additional quantity of the good; its expression necessarily must contain the *unit* of the good in question" (pp. 4-5).

[16]Why then are neoclassicals admittedly more inclined than Misesians to make continuity-related mistakes? The answer is that the vastly greater number of neoclassical economists leads to greater division of labor, with some economists devoting their lives to the study of continuity while the remainder get down to other business. Continuity-related mistakes largely arise due to the low level of communication between specialists—a sociological rather than a doctrinal failing.

[17]Mises attaches the disparaging proviso that such curves "do not add a whit to our insight," but goes on to note that "such curves may prove expedient in visualizing the problems for undergraduates" (1966, p. 333). At minimum, then, these curves *can* "add a whit to the insight" of undergraduates!

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Income and substitution effects

Caplan (1999) maintains that Rothbard contradicts himself by introducing income effects and backward-bending supply curves after purporting to prove that the laws of supply and demand are exceptionless theorems. Block denies the charge; Rothbard is perfectly correct, given the "*ceteris paribus* assumption of no income changes" (1999, p. 29). There is a fundamental problem with Block's reply, though. It hardly makes sense to invoke an "all else equal" condition in cases where all else is *of necessity never equal*! The key neoclassical insight is that price changes ipso facto change income. Income effects do not happen at the same time as price changes by miraculous coincidence. They are inherent in the nature of price changes. Block's defense of Rothbard makes about as much sense as a "theorem" that "no one ever dies of starvation," which is apodictically true "given the *ceteris paribus* assumption that no food is nourishing."

Hülsmann, in contrast, amazingly declines to either (a) defend Rothbard's use of income and substitution effects, or (b) argue that this was an unfortunate neoclassical corruption of Rothbard's thinking. Instead, he remarks only that "Mises did not bother about the shape of supply curves" and that relatively little depends on this.[18] Bear in mind that "shape" here is not just slope, but sign! "The fact that Rothbard occasionally refers to income and substitution effects does not warrant the claim that these effects correspond to anything real" (Hülsmann 1999, p. 11). True enough; but Rothbard did claim to have strictly deduced the conclusions that supply slopes up and demand slopes down from the axiom of action. Why doesn't Hülsmann say that Rothbard's supposed proofs *warrant the rejection* of the income and substitution effects? If he thought Rothbard's proofs were valid, he would. And if Rothbard's proofs of the laws of supply and demand are not valid, the remaining ten chapters of *Man, Economy, and State*—everything from interest-rate determination to monetary economics to the theory of price controls—rest upon error. If these are not "fundamental feature[s] of price formation" (Hülsmann 1999, p. 11), what is?

Demonstrated preference and welfare economics, I: Block

Block misinterprets my paraphrase of Rothbard's utility theory: "[T]he free enterprise system only *permits* the implementation of all desired voluntary

[18]In fact, Mises did "bother" about the shape of supply curves. The following quote plainly takes the positive slope of supply curves as a given:

> The monopolistic buyer—whether he is an individual or a group of individuals acting in concert—cannot reap a specific gain corresponding to the monopoly gains of monopolistic sellers. If he restricts demand, he will buy at a lower price. But then the quantity will drop too. (1966, p. 383)

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exchanges, this is a far cry from guaranteeing any such outcome. Caplan appears to assume the latter, however" (p. 32). But this ignores a key proviso in my original statement: "*at that moment* at least one of them would not have benefited" (Caplan 1999, p. 833; emphasis added). Yes, as Block points out, two people might fail to trade solely because they don't *know* of each other. But praxeologically speaking, this only means that more mutually beneficial trades could have happened if conditions (in this case, communication) were somehow better.

Block moves into more interesting ground in his discussion of *verstehen*, or understanding. For what is *verstehen* other than what Hülsmann calls "applied psychology"? I wholeheartedly agree with Block's observation that "The economist must *interpret* human action; he must 'get into the mind' of the economic actor" (1999, p. 33). My complaint is, in essence, that Misesians do not take *verstehen* far enough. If *verstehen* works for signing a contract, it works for envy, too. Block's assertion that "[W]e *know* what a contract signing means. But in the case of supposed envy, we really do not" (1999, p. 33) is bizarre. Surely Block will not claim apodictic certainty in the former *verstehen*; so why is the lack of apodictic certainty a problem for the latter? What is the difference? Accurate *verstehen* may be harder for envy than contracts, but there is no way for Block to justifiably treat these as radically different cases. The same applies to judgments about public goods, efficiency, and so on. We identify them using "applied psychology" or *verstehen*, hopefully after careful study of the facts. Such judgments are fallible, but the same is true of all *verstehen*.

Finally, contrary to Block, there is nothing *inherent* in public goods theory that makes it an "ideological wedge for government intervention" (1999, p. 36). As Caplan (1999) mentions, public choice economists have used public goods theory to analyze the inefficiencies of democracy. David Friedman (1989, pp. 156-59) bases much of his argument for anarcho-capitalism squarely on public goods theory! In my own research on the economics of irrationality, I use public goods theory to explain why democracy is so inefficient compared to the market (Caplan 2001b). Most economists are statists, so they unsurprisingly tend to put a statist spin on various "failures" that apply just as well–if not more so–to the state itself.

Demonstrated preference and welfare economics, II: Hülsmann

Hülsmann asserts that "'efficiency' refers to the relationship between means and ends. . . . All one can state is that [a policy] is efficient for some persons and inefficient for others" (1999, p. 16). This is, for once, a purely definitional disagreement. Hülsmann addresses *means-ends* efficiency. But I was clearly referring to *Kaldor-Hicks* efficiency, the most common usage; and this sense of the term is not agent-relative (Posner 1998, pp. 12-17; see also Landsburg

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1993, pp. 49-105). *Kaldor-Hicks* inefficiency sidesteps the problem of "comparing non-extended entities like value, utility, preference ranks, etc." (Hülsmann 1999, p. 16) by instead comparing willingness to pay (Stringham 2001). As an absolute ethic, I would surely reject this efficiency standard, but it is internally coherent.

Hülsmann may be correct that "the original purpose of public goods theory was to establish a rational criterion for government intervention"[19] (1999, p. 17). But like many other theoretical innovations, the *motive* behind its development is irrelevant to its truth. Even if the "original purpose" of atomic theory was to build better bombs, this neither shows that atomic theory is false, nor that it lacks civilian applications.

Finally, I am well aware of Hoppe's "argumentation ethics" (Hoppe 1989, pp. 127-44). I did not discuss it in my original article because my goal was to critique Austrian economics at its strongest. Like almost everyone who has walked through the steps of Hoppe's attempted proof, I see it as a tissue of error (Friedman 1988, Lomasky 1989). At an absolute maximum, Hoppe shows that arguments in favor of violations of private property *during arguments* are self-refuting.

Conclusion

Block aptly sums up much of our disagreement:

> The Austrian takes a position midway between that of the behaviorist and Caplan's neoclassicism. For the behaviorist, there is no such thing as motive, introspection; all is human behavior. For Caplan, all is motive and introspection; no behavior whatsoever is necessary to tie these phenomenon to the real world. In the praxeological perspective, there is both, and the latter is necessary to demonstrate the former. (1999, p. 24)[20]

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[19]I doubt, however, that economists who do see it as a rational criterion for intervention would find Hülsmann's counter-arguments impressive.

> [E]xternalities are clearly not a feature of a good as such, but depend exclusively on the subjective feelings of those other people. Whenever any person other than its owner takes an interest in a good, it becomes *ipso facto* a public good. As a consequence, there is no means to clearly distinguish between public and private goods. (1999, p. 16)

Hülsmann's foils would presumably reply that the distinction is clear enough and stable enough for practical purposes.

[20]This passage stands in contrast to Rothbard (1962, p. 266): "[I]n so far as praxeology touches on psychology, its principles are the *reverse* of those of behaviorism."

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The crux of the disagreement comes in the final clause. Yes, there are innumerable connections between introspection and behavior, but are the latter necessary to demonstrate the former? Nothing could be further from the truth. Individuals can know their own motives from introspection alone. And unless Block is talking about absolute certainty, common sense tells us that we can also know a great deal about the motives of others without ever observing their behavior. By rebuilding economics on the denials of these truisms, Misesians are moving off the path of realism rather than along it.

What could possibly motivate this queer refusal to recognize the reality of most mental states? This is a serious challenge for my *verstehen*, and a dose of humility is in order. Still, my best guess is that it hinges on the Austrian rejection of probability theory. When apodictic proof is unavailable, they have no substitute to fall back on. Orthodox probability theory, in contrast, formalizes the common-sense view that certainty is just the endpoint on a continuum. A person speaks the words, "I envy the rich." No one can *prove* he is telling the truth. For the Misesian, this insight virtually ends discussion. But common sense can still inquire, "*How likely* is the speaker to be telling the truth?" It is not a defect of public goods theory that there "exists no unambiguous way to measure the costs and benefits" (Block 1999, p. 36) of, for example, education. This reflects the inherent uncertainty of the world. The common-sense response is to put forward one's best estimate. By treating all imperfect answers as equally bad on methodological grounds, the Austrians are once again straying from the path of realism.

To end on a positive note: Block's practice of introspection is far better than his theory. Caplan does indeed agree that "[Austrianism] has far more to offer the practitioner of economics than [Marxism]" (Block 1999, p. 37). But the Austrian contribution comes not from the letter of its doctrine, but from the spirit of its approach. In methodological practice—though never in pure theory—neoclassical economists have indeed often been narrow behaviorists. Their firewalls against survey evidence and narrative history have only recently begun to crumble. Rothbard (1960) saw through their cant from the start. Austrians have also long recognized the robustness of the free market: Rothbard anticipated the theory of contestable markets (1962, pp. 573-79, 787-91, 913 n. 72; 1977, pp. 37-80) as well as rational-expectations macroeconomics (1962, pp. 683-87, 864, 875-77; 1963, pp. 43-53), and he seriously undermined the *application* of public goods theory to the defense services industry (1978, pp. 215-41). Finally, few neoclassicals have analyzed democracy with the Austrians' lack of reverence (Rothbard 1977, pp. 16-23, 189-99). Public choice economists frequently try to temper dispassionate analysis of democracy's failings with abiding faith in its sanctity. Austrian critiques of democracy have consequently often been more penetrating.

Block also astutely parses my left-handed compliment to mainstream economics: "this is a bona fide case where neoclassical economists did not merely

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tediously formalize the obvious" (Caplan 1999, p. 829). I have no illusions about the quality of most research. Austrians are quite prudent to avoid the modal neoclassical publication.[21] Only a small percentage are worth reading. But one should not jump to the conclusion that the total contribution of neoclassical economics is small. The modern literature is so massive that multiplying it by the tiny percentage of insightful articles yields a large body of valuable knowledge. Yes, most neoclassicism is bad economics, but most good economics is still neoclassical.[22]

There are two paths for Austrian economics to evolve along. The first is obscurantist philosophizing about preferences, probability, uncertainty, and welfare economics. The second is to reinvent itself as a species of neoclassical economics infused by the spirit of the Austrian approach. Yes, there are many neoclassical economists who focus myopically on "measurable behavior," who insist that markets *only work* if we assume "perfect competition, perfect information, and no externalities,"[23] and who turn a blind eye to every failing of democracy. But the problem is not neoclassical economics; it is bad neoclassical economi*sts*. By becoming good neoclassical economists, the Austrians can help shift the balance.

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[21]Then again, neoclassicals are likewise prudent to avoid the modal Austrian publication. While Austrians are rarely guilty of tedious *formalization*, they regularly engage in tedious *repetition* of what Mises or Rothbard said, or, worse yet, tedious exegesis of what Hayek meant to say.

[22]For a discussion of the main postwar achievements of neoclassical economics, see Caplan (1997).

[23]Contrary to many classroom presentations, these are merely *sufficient*–not necessary–conditions for the Kaldor-Hicks efficiency of laissez-faire. For example: Bertrand competition and contestable monopolies work as well as perfect competition; *symmetric* imperfect information implies no deviation from efficiency; and externalities have no efficiency implications if they are infra-marginal, or if positive and negative externalities balance each other out.

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FIRE AND SMOKE: GOVERNMENT, LAWSUITS AND THE RULE OF LAW. BY MICHAEL I. KRAUSS. SAN FRANCISCO: THE INDEPENDENT INSTITUTE, 2000.

This monograph by Professor Michael Krauss of the George Mason University School of Law is a well-written and accessible critique of the recent government lawsuits against the tobacco and firearms industries. The first of these so-called recoupment suits was filed in May 1994 by the attorney general of Mississippi against the tobacco industry for the recovery of Medicaid and other expenditures by the state for medical treatment of sick smokers. Four years later, forty-six states and a few territories entered a settlement agreement with the tobacco industry following earlier settlement agreements between the industry and the states of Mississippi, Florida, Texas, and Minnesota. In 1999, the federal government filed a similar lawsuit against the tobacco industry. Since 1998, dozens of municipal governments have filed lawsuits against the firearms industry for the recovery of municipal expenditures for police departments, the criminal justice system, and the medical treatment of persons injured in firearms-related incidents.

These recoupment suits have been criticized by many conservatives and libertarians, who have described these lawsuits as, among other things, "executive taxation," "regulation through litigation," and "extortion." Professor Krauss's monograph is a primer from that perspective. Krauss explains how these recoupment suits are based on a general misapplication of private law to public disputes, and he discusses the specific legal and economic flaws of these lawsuits.

The early conservative and libertarian scholarship on this subject uniformly criticized the tobacco litigation. In 1996, I chaired a committee of five attorneys, including Professor Michael DeBow of the Cumberland School of Law of Samford University, to consider whether the State of Alabama should file a recoupment lawsuit against the tobacco industry. That committee unanimously recommended against the filing of a tobacco suit, and the committee published a lengthy report, which was later published in a law review. Several months later, Professor DeBow wrote a policy study of the tobacco suits which was published by the Heartland Institute, and later that same month, Robert Levy wrote a similar policy study, published by the Cato Institute.

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As recoupment litigation multiplied, so did the critical commentary. In 1999, the U.S. Chamber of Commerce, with cosponsors The Federalist Society and The Manhattan Institute, hosted a major conference about recoupment litigation, and in 2000, the American Tort Reform Foundation published a monograph on the subject, written by John Fund of *The Wall Street Journal* and Martin Wooster of *The American Enterprise*. The general counsel of the American Tort Reform Association, Victor Schwartz, drafted and touted model legislation to curb the abuses of this new litigation. Much of this commentary focused critically on the retention of politically active trial lawyers to represent government agencies on a contingent-fee basis or, as Robert Levy described it, "private lawyers using the power of the state to enforce public law—with an incentive to increase the penalties."

Professor Krauss's contribution to this scholarship and commentary is two-fold. First, Krauss briefly explains the key distinction between public and private law. Krauss notes that, in contemporary American culture, "[p]ublic law, involving relationships between citizens and the state, is all the rage. Constitutional litigation makes headlines, as it should. And criminal trials are often appropriate front-page fodder" (p. 2). Krauss explains, however, that private law, not public law, is "what distinguishes free societies from totalitarian ones. All countries have public law institutions. But only in free countries does private law govern the acquisition and exchange of rights" (p. 2). To blur this distinction and transform the private law of torts into a form of public law ultimately means that "freedom is replaced by collectivism" (p .5).

Krauss then proceeds to his second point: The government recoupment litigation against the firearms and tobacco industries is flawed as matters of both law and public policy. Krauss explains why public concerns of the governmental costs attributed to gun violence and tobacco-related illnesses are not properly the subject of the private law of torts. He explains why each of the claims, under traditional tort law, fail because either the alleged conduct of the defendants was not legally wrongful or the alleged victims, such as smokers, voluntarily assumed the risks of harm to themselves. In the gun context, Krauss refers to persuasive studies that report the benefits of firearms ownership by law-abiding citizens and law enforcement agencies outweigh the costs of gun violence. In the tobacco context, Krauss again notes that leading economic studies show that "smoking does not cause any economic loss to government" (p. 26), because smokers pay high taxes and, through premature deaths, impose fewer pension and medical costs on the welfare state.

Krauss concludes by suggesting two ordinary public-law alternatives for the resolution of these matters: taxation and prohibition. Krauss explains that neither alternative is likely to succeed, and for that reason, elected representatives are reluctant to turn to these public remedies. That is why the proponents of government expansion have turned to litigation to achieve their goals, as an end-run around the political process.

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There is one argument advanced by Krauss with which I am sympathetic, but I find unavailing. Krauss contends that the recent multistate tobacco settlement "contravenes antitrust law and quite possibly the Constitution" (p. 29). Although I certainly agree that the national tobacco settlement is a flawed policy that strengthened the ability of the tobacco industry to behave as a cartel, the agreement with the state governments removed the settlement from the ambit of the antitrust laws. The legal challenges filed by tobacco distributors, which are based on the same arguments advanced by Krauss, have failed repeatedly.

Much of what Krauss says about recoupment litigation has been said elsewhere, but Krauss presents this subject better than most. He cogently explains both the big picture of why this litigation is a threat to liberty and the specific legal and policy flaws of each lawsuit. For any friend of liberty who wants to learn more about this subject, Krauss offers, in forty pages, one of the best starts available.

BILL PRYOR
ATTORNEY GENERAL OF ALABAMA

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THE THEORY OF MONETARY INSTITUTIONS. BY LAWRENCE H. WHITE. OXFORD: BLACKWELL PUBLISHERS. 1999.

Austrian economist Lawrence White, formerly of the University of Georgia, has given us in *The Theory of Monetary Institutions*, a book written in mainstream mode yet embellished with Austrian insights. Based on lectures given by Professor White in the courses he teaches, and furnished with thought-provoking questions at the end of each chapter, the book is intended for classroom use–if not as a text, then at least as a supplement for graduate and upper-level undergraduate courses in monetary economics. A recurring theme of this book is that most of the actions governments everywhere have taken to influence monetary affairs are devoid of economic justification.

The book's thirteen chapters can be grouped into four broad categories: the origins of monetary institutions, the role government should play in monetary matters, an analysis of the government's actual behaviors, and a look at possible future monetary institutions. The first of these sections has the greatest Austrian content, as it is based largely on Carl Menger's account of the origins of money. White makes clear that the notion of differing degrees of marketability of goods is the *sine qua non* for understanding the evolution of money. He also notes that this notion is absent from the Walrasian general-equilibrium tool kit. From there, White goes on to discuss the origins of coinage, banknotes and deposits, reciprocal acceptance of notes at par, and clearing arrangements. His analysis leads him to the conclusion that none of these mechanisms required the heavy hand of government to create them.

His discussion of privately produced money reflects the advocacy of free banking seen in his earlier work. It is not surprising, given this orientation, that his estimate of the resource cost of a gold standard differs from Milton Friedman's, largely in that White assumes a fractional reserve system (2-percent reserves against demand liabilities), while Friedman's is based on 100-percent gold reserves. White shows the ability of the classical fractional-reserve gold standard to deliver a 0.5 percent average annual decline in prices as compared to a 6.5 percent increase in more well-behaved fiat money systems (inexplicably, he excludes cases of hyperinflation, although they, too, can result from fiat money) makes a gold standard well worth the resource costs

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incurred in its operation. However, in doing so, he follows the standard practice of equating the welfare cost of inflation to "the amount by which the dollar value of lost consumer surplus exceeds the gained government revenue from monetary expansion" (p. 48). A more thoroughgoing Austrian critique would not have accounted the additional government revenue a benefit of fiat money.

His section on the role of government in money and banking methodically demolishes pretext after pretext used even by otherwise free-market-oriented economists who claim money to be an exception to the general case for laissez-faire. Thus, he explains why money is not a public good, exhibiting both excludability in supply and rivalness in use. But he doesn't stop there. Enthusiasts of government provision of money who have conceded this point have taken the fallback position that although money itself may not be a public good, monetary stability, or uniformity, or information about money nonetheless are. Professor White dispatches each of these contentions swiftly. The argument that money has external benefits that prevent markets from producing money in an optimal manner is another which fares no better at his hands. Thus, in response to Stephen Morrell's argument that government could have quickened the process of convergence to a single medium of exchange by publicizing which goods are most marketable, White retorts in fine praxeological fashion that,

> to improve the market outcome, those in government have to know better than those in trade what is the most salable good among traders. Further, to speed the emergence from barter, those in government would have to know that the market process is heading toward convergence on a commodity money, even though they live in an economy in which money has never existed. (p. 94)

Touché!

The book's discussion of actual government behavior in the monetary sphere examines the implications of different factors motivating these actions, including the accumulation of government revenue through seignorage, maximizing the power of the bureaucrats in charge of monetary policy, and manipulating the business cycle to enhance the electoral prospects of incumbent officeholders. Of these, I found the discussion of seignorage most interesting, with its explanations of how governments can maximize their take, why they usually stop short of abusing that power to the fullest extent possible, and how financial regulations enhance the ability of government to obtain seignorage. What was disappointing in this discussion was the author's failure to note that creating money to raise revenue in this way is tantamount to counterfeiting. The closest he comes is how note holders of base money are cheated if money expansion raises prices, as it invariably must (p. 160).

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One of the virtues of this book is its organization. By and large, the chapters flow logically from one to the next, as the author pushes many a line of thought to its ultimate conclusion. For instance, his chapter on political business cycles leads to a discussion of rules vs. discretion, which culminates in his making the case that the abolition of the Fed would be a prerequisite to obtaining the benefits of a rule-based monetary regime (p. 222).

The question of monetary constitutions is the locus of one major omission, especially from someone with Professor White's Austrian pedigree. Here he mentions three schools of thought–discretionary central banking, constitutional central banking, and free market-free banking (pp. 218-19). Conspicuous by its absence, however, is an alternative near and dear to the hearts of many Austrians: the 100-percent-reserve gold standard. Clearly, he is familiar with it, having himself critiqued that point of view in the predecessor to this publication.[1]

That said, this volume's virtues far outweigh its failings. Despite some necessarily involved math used to elucidate some of the models he scrutinizes, *The Theory of Monetary Institutions* boasts a lucid, even lively, writing style. Moreover, Professor White successfully meets the challenge of not letting the equation trees obscure his ability to enable the reader to grasp the economics forest. Furthermore, he deftly weaves historical material into his theoretical discussions, recognizing that such episodes are what theory seeks to explain. This book's broad scope should leave those students who use it well-versed in a full range of monetary issues and well-equipped to refute arguments which suggest that only government is capable of maintaining a properly functioning monetary system.

ROBERT BATEMARCO
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[1]George Selgin and Lawrence H. White, "In Defense of Fiduciary Media–or, We Are Not Devo(lutionists), We Are Misesians!" *Review of Austrian Economics* (1996): 83-107.

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MAKING SENSE OF SOCIAL SECURITY REFORM. BY DANIEL SHAVIRO. CHICAGO: UNIVERSITY OF CHICAGO PRESS, 2000.

IS SOCIAL SECURITY BROKE? A CARTOON GUIDE TO THE ISSUES. BY BARBARA R. BERGMANN. ANN ARBOR: UNIVERSITY OF MICHIGAN PRESS, 2000.

ADMINISTRATIVE ASPECTS OF INVESTMENT-BASED SOCIAL SECURITY REFORM. JOHN B. SHOVEN, ED. CHICAGO: UNIVERSITY OF CHICAGO PRESS, 2000.

With more young people believing in UFOs than future returns from Social Security, and exit polls in the November election in Florida showing a slight majority of senior citizens choosing Bush over Gore, it seems that the Bush Campaign 2000 touched the Third Rail of American politics and came away relatively unharmed. The Third Rail aside, the closeness of the election, combined with an eroded Republican congressional majority, means that a significant reform of the current system is unlikely. Given the spirit and specifics of the Bush proposal, that is a good thing.

The *cognoscenti* behind the Bush proposal call their plan "privatization." Privatization, as typically understood by economists, means the transfer of capital ownership and resource allocation from the state to private individuals or organizations. Social Security is a tax-revenue stream from workers to dependents. Thus true privatization would involve repealing the FICA tax and allowing individuals to save or spend the attendant increase in income however they wished. Is this genuine privatization the vision of the academic-Beltway brain trust behind President Bush's and others' proposals for Social Security reform? Or is it their intention to convert the Third Rail into a Third Way? Two recent "pro-privatization" volumes—the first authored by Daniel Shaviro and the second edited by John B. Shoven—will unavoidably lead the thoughtful reader to conclude the latter.

Daniel Shaviro and Barbara Bergmann have written primers on Social Security and its reform. Shaviro, a law professor at New York University, first reviews what he calls "traditionalist" reform proposals favored by Brookings

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types such as Henry Aaron. The traditionalist aim is to put the system on firmer financial footing while "preserving its fundamental character." The aspects of this much-admired "character" include universal coverage, wealth redistribution, and a uniform "benefit package." Shaviro is bothered by the lack of choice in benefits. He states that the poor and those with no planning ability are worse off compared to financially savvy savers who have a wide choice of supplemental investments. It is telling that these criticisms could also be effectively leveled at universal coverage, but Shaviro is not bothered by this.

The first version of traditionalist reform that Shaviro reviews is that consisting mainly of tax and benefit changes for the purpose of keeping the current system financially afloat. Shaviro does not cite Bergmann, but she clearly falls into this first camp of traditionalists. Bergmann sees the entire "privatization" movement as a conspiracy by Wall Street interests and "folks who hate the idea of the federal government" (p. 52). There is at least a grain of truth to this, like most of the arguments she advances. She seems to be eminent among traditionalists (although not among Democratic propagandists) in her belief that fixing Social Security's solvency problems through tax-benefit tinkering would be a cakewalk. Although she notes that an increased tax burden would be felt by Generations X and Y (p. 26), she seems quite unconcerned about the "unfairness" of this—although she is quick to play the intergenerational "unfairness" card vis-à-vis "privatization" (p. 70).

The second (and more daring) version of traditionalist reform that Shaviro reviews is represented by Robert M. Ball. Ball advocates that portions of the Social Security Trust Fund be invested in the stock market by the government swapping its debt in open markets for equities. This raises the problem of direct government ownership of private corporations. Unlike even Bergmann, Shaviro is untroubled by the tremendous danger inherent in such a plan. Direct ownership of securities by the federal government should not be too worrisome, he contends, since it should depend on one's view whether one wants more or less intervention in the economy. In other words, the failures of pure socialism are the stuff of some people's opinions, not an objective reality per se. One can hardly imagine the average North Korean having the luxury of indulging in such bizarre delusions.

One of the most irritating features of Shaviro's treatment is his constant attempt to equate government-directed processes to market-directed processes. Two of myriad examples: Increasing the FICA tax could end up "increasing national saving" (p. 103), and individual accounts arguably exist in today's Social Security system because Social Security tracks individual tax and earnings data (p. 145). The latter is a stunning statement for a law professor, forgetting that in *Flemming v. Nestor* (1960), the Supreme Court ruled that workers have no individual legal right to Social Security.

Opposing privatization's "opposition to progressive [wealth] redistribution" because it "is not self-evidently correct" (p. 152), Shaviro proposes a

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Third Way he calls "progressive privatization." Unsurprisingly, it includes taxes, "significant limits on consumer choice" of investments (p. 153), "progressive [wealth] redistribution" (p. 153), required purchase of annuities at retirement, and limited inheritability of account balances. This of course is nothing more than a slightly more generous tinkering to the system than that favored by Ball-type traditionalists.

Much more sane and readable is a volume of papers from a 1998 NBER conference on Social Security reform. John B. Shoven, the volume's editor, notes the advantages of individual Social Security accounts: real assets can build and accumulate over time, unlike the current trust fund, and the increase in personal and national saving implies a higher standard of living for workers in the long run as wealth previously transferred from one generation to another earns a real rate of return from investment.

The fly in the ointment is that 140 million individual accounts will be extremely costly to administer. These huge costs could negate all the advantages of a private system. The first four papers in this volume focus on setting up the least-costly system of individual accounts.

Fred T. Goldberg, Jr., and Michael J. Graetz seek to keep costs down by suggesting that the new system administering the individual accounts be built on the foundation of the current wage-reporting, payroll-tax schemes to collect funds and track account accruals. This new structure, built on the old system, would add little in terms of additional costs. This saves costs on the collection and clerical side. What is the answer to investment and distribution? A simple, no-frills collection of private funds selected by the government. Other private mutual funds offering a wider range of services and investment selections would be authorized to compete with the basic government plan. Goldberg and Graetz amusingly believe that the government plan would provide a competitive "discipline" to the ancillary private plans.

Sylvester J. Schieber and John B.Shoven look at privatized and partially privatized Social Security systems in Chile, Australia, Britain, and Sweden for guidance and insight. Proving that administrative costs are comprised of a high proportion of fixed costs, the twenty-seven-year-old system in Australia had average administrative costs of 0.8 percent in 1997, with costs steadily decreasing as the average account's size increases. Schieber and Shoven believe this augurs well for the U.S., given the much more developed and competitive nature of U.S. capital markets. To keep record-keeping costs down, Schieber and Shoven advocate the establishment of a PSA (personal security account) Central run by the Social Security Administration or a private firm under contract management. Echoing Goldberg and Graetz, they advocate the establishment of a low-risk, no-frills government plan operating alongside other more risky plans with various investment bells and whistles.

Estelle James, Gary Ferrier, James Smalhout, and Dimitri Vittas compare the costs of three alternative systems. The first consists of individual accounts

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invested in the retail market (à la 401[k]s); the second consists of individual accounts invested in the institutional market, with limits on choice among fund companies; and the third consists of no individual accounts but a large central pool of funds invested in the institutional market. Using an econometric model, James et al. find that under the retail alternative, investors benefit from economies of scale but face high marketing costs. The last alternative (central pool of funds) is the least costly, achieving economies of scale sans marketing but eliminating worker choice of investments. It is also subject to politicized allocations of capital. The second choice, individual accounts institutionally invested, seems to be the Goldilocks "just right" to James et al., since it allows worker choice, involves economies of scale without high marketing costs, and doesn't run the risk of politicized investment.

Peter Diamond champions something similar to the pool of funds proposal of James et al. because of the supposed cost advantage. Using the federal employees' Thrift Savings Plan (TSP) as his guide, contributions of 2 percent of earnings would have average administrative costs of 0.4-0.5 percent of assets per year over a forty-year working career. Based on his analysis of Chile and U.S. mutual funds, he concludes that the cost of private accounts would be at least twice as high.

Shoven, the editor, admits that "[n]one of the papers evaluates a completely unregulated, fully private system for cost and, presumably, other reasons" (p. 6). This is sensible in light of the fact that evaluation of such a system obviates the pretense of the volume (i.e., solving the "problem" of high costs). It signifies that the authors have painted themselves into a flimsy logical box. Embracing universal forced saving guarantees a centralized government scheme. It then comes as no surprise that, assuming centralization, the government emerges as the most cost-effective administrator—thus the complete logical circularity of the papers in this volume. Wryly amusing are the contortions undertaken by analysts with market predilections to derive market-dominated systems from statist assumptions. Sad will be the consequences if these disasters are ever implemented.

There is much sanctimonious grandstanding about one benefit of "privatization": offering workers a choice in terms of asset holdings. But this is a selective advocacy of choice, since choosing to opt out of the system entirely is denied. Hence, those with high time preferences for consumption (including most of the underclass poor with shorter average life expectancies) are made worse off under forced saving. This is exactly where the Third Way house of cards collapses. It finds problems to solve in its paternalistic box, but it can't find a consistent justification for the box itself. As Lord Francis Jeffrey said after reading Wordsworth, "This will never do."

DALE STEINREICH
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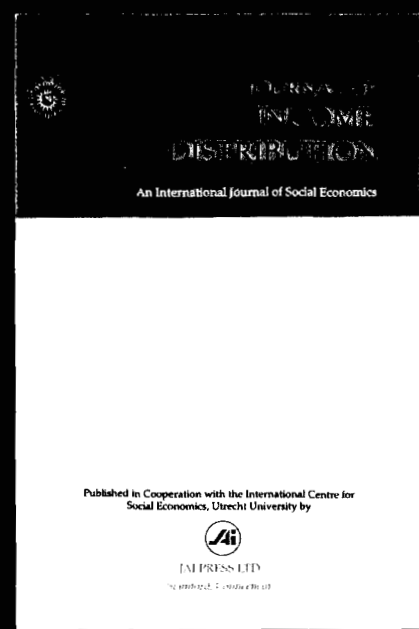
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